# A Dual Perspective on the Economics of Sports

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# Contents

**Acknowledgements** **V**

**Framework paper** **1**

**A Appendix: Papers included in this dissertation** **11**

A.1 Managers' external social ties at work: Blessing or curse for the firm? . . 13

A.2 Tie-transfers and player performance in professional soccer . . . . . . . . . . . . 51

A.3 Does sports activity improve health? . . . . . . . . . . . . . . . . . . . . . . . . . . . . . . . 85

**Curriculum vitae** **117**

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Zurich, January 2015 Marc Brechot

# Framework Paper

A remarkable expansion of the field of studies can currently be witnessed in economics as many areas previously considered to belong to sociology, psychology, and other sciences are drawing the attention of economists. One area that has been assimilated in the course of this expansion is the economic analysis of sports. Originally, the motivating idea behind this approach was to apply a “...dose of economic thinking to the business of sports” (Fort, 2003). Indeed, research has shown that economics can greatly help the sports industry by providing insights into the sports labor market, the demand for sports, the institutional design of sporting contests, and many other related topics.

However, in recent years it has been realized that the economic analysis of spectator sports can also be viewed from the opposite perspective, and be used to illustrate universal economic principles (Rosen & Sanderson, 2001). The logic of this new approach is that spectator sports provides in many ways a perfect laboratory for studying economic questions: an abundance of data is readily available, the goals of the actors are often straightforward, the stakes are typically high, the subjects are experienced professionals, and the outcomes are extremely clear (Palacios-Huerta, 2014).

Following this idea, game theory has been tested using soccer players’ penalty kicks and tennis players’ serves (Chiappori, Levitt, & Groseclose, 2002; Walker & Wooders, 2001). Prospect theory has been tested using golfers’ decision-making on the green (Pope & Schweitzer, 2011), and important insights on the resource-based view of the firm have been gained from studies of professional team sports (Berman, Down, & Hill, 2002; Holcomb, Holmes Jr, & Connelly, 2009). The common thread of all these highly-recognized studies is that they draw conclusions on economic theories based on human behavior observed in professional sports.

This dissertation takes first one vantage point, then the other in employing the economic analysis of sports to ask both what sports can learn from economics and what economics can learn from sports. Since the economic literature devoted to the study of sports is still in its infancy, there are countless interesting and unanswered questions that can be asked from each of the two perspectives: new questions on the labor market for professional athletes and the governance and regulatory actions of sports governing bodies have arisen as sports clubs and competitions have globalized in their search for greater performance and exposure. The rise of sports-related industries such as broadcasting or betting also demand to be studied more closely. And the only limit to what economics can learn from sports seems to be its own ability to find questions to which the plethora of data from sports can give an answer.

The greater objective of this dissertation is to address a few carefully selected questions having *a dual perspective on the economics of sports* in mind: illuminating the world of sports through economics and making progress in the world of economics through sports. The selected questions are individually addressed in three empirical papers.[1]

The first paper, *Managers' external social ties at work: Blessing or curse for the firm?* (see Appendix A.1), investigates how managers' history of social relations influences firm-level decision-making and overall firm performance. While previous research has shown that newly formed social ties to internal coworkers can lead to favoritism and reduced firm performance (Bandiera, Barankay, & Rasul, 2009), we examine the question of whether social ties to others outside the firm can interfere

[1]The introductory words of this dissertation were inspired by *Beautiful Game Theory: How Soccer Can Help Economics*, a recently published and excellent book by Ignacio Palacios-Huerta (2014).

with employees' optimal selection of transaction partners and thereby harm firm performance

To address this research question, we use the sports industry as a laboratory because professional sport offers an ideal setting to study the effect of managers' external, social ties: "There is no research setting other than sports where we know the name, face, and life history of every production worker and supervisor in the industry ... and [where] we have a complete data set of worker-employer matches over the career of each production worker and supervisor in the industry" (Kahn, 2000, p. 75). In line with this idea, we use 34 years of data from the National Basketball Association (NBA) and combine each team's record of player acquisitions and sporting performance with data on the working history of its key decision maker: the general manager.

In our empirical analysis, we study the relationship between a team's winning percentage and its use of players acquired by the general manager from his former employers in the NBA. We find that teams with such tie-hired players underperform teams without tie-hired players by a substantial 11 percent. Subsequent regression analyses reveal that this difference in winning percentages cannot be explained by adverse selection of general managers and teams into the use of tie-hiring procedures. Rather, we show that agency theory and private, tie-related benefits for the general managers best explain the negative effect because tie-hired players reduce team performance only if they have been acquired in the presence of low monitoring incentives for team owners. In contrast, if properly monitored by the owner, general managers are found to be less likely to engage in such moral hazard behavior.

Overall, the paper demonstrates that – in the absence of appropriate performance incentives – social ties to others outside the firm can undermine employees' decision-making on behalf of the firm. This finding has important implications for the management and economics literature. Previous studies have found employees to make tie-influenced decisions for the firm in connection with hiring (Fernandez & Weinberg, 1997; Williamson & Cable, 2003) financing (Shane & Cable, 2002), or investing (Cohen, Frazzini, & Malloy, 2008). Anecdotal evidence also seems to suggest that firms often seek well-connected employees. Our results show the hidden costs of such hiring practices, and reveal the novel finding that managers' tie-influenced decisions-making can lead to a discriminating assessment of external transaction partners and can sometimes become a curse for the firm.

The second paper, *Tie-transfers and player performance in professional soccer* (see Appendix A.2), examines the effect of tie-influenced transfer decisions in professional soccer. Conventional wisdom suggests that interpersonal networks have always played an important role in clubs' decision-making in the transfer market. However, little is known about what is really going on in the transfer market. As noted by soccer writer A. Thomas (2014): "It's too big, and too disparate, and has far too many people and far too much money moving around to be truly comprehensible". Nevertheless, as the global transfer market in professional soccer has become big business in the last few decades, it is important for all actors in the game to better understand the influence of interpersonal networks on transfer decision outcomes.

Drawing on economic literature on the referral-hiring phenomenon and the effects of social ties, this paper provides insights on one prominent form of interpersonal network that plays an important role in clubs' decision-making in the transfer market: ties between coaches and players created by a past working relationship. Such coach–player ties are supposed to mitigate search frictions in the recruitment of players because coaches have first-hand information about the players they have previously worked with. But the possibility to improve transfer decisions through this channel strongly depends on the coach's ability to screen its player network effectively. Existing economic evidence on job referrals indicates that this ability cannot be taken for granted because employees often lack a good understanding of which network members will perform better and are unable to make good referrals for their employers even when properly incentivized (Beaman & Magruder, 2012).

Using employment records to identify past working ties between coaches and players, we analyse over 2,300 transfer decisions from clubs in the German Bundesliga in the period from 1995/96 until 2013/14. We find that players acquired through transfers where coach and player have previously worked together outperform players acquired through other transfers by 3 percent. Hence, previous coach–player ties can be a valuable resource for clubs' transfer market activities. However, extended analyses show that not all transfers involving a coach–player tie are equally desirable. Transfers where a new coach brings a player straight from his previous club result in an underperformance of the newly acquired players of almost 8 percent. Given the strong disciplinary power of the labor market for professional soccer coaches (see e.g., Barros, Frick, & Passos, 2009) and the resulting strong incentives for coaches to make "good" referrals, it seems that coaches mistakenly make "bad" referrals on players with which they have worked together just recently at their prior clubs.

Taken together, the results from our study reveal that it is highly important to know the contextual factors for an effective use of referral-hiring practices in professional soccer. While coach referrals can represent an attractive way of finding out the club-specific value of players in many contexts, clubs have to be cautious when a newly hired coach wants to bring players straight from his previous club because such referrals result (on average) in poorer transfer decisions. Hence, coaches (and clubs) tend to underestimate certain contextual factors when hiring players straight from their prior club.

The third paper, *Does sports activity improve health? Representative evidence using proximity to sports facilities as an instrument* (see Appendix A.3), investigates the effect of leisure-time sports activity on health and health care utilization. Most previous research on leisure-time sports activity in social sciences has come from sociology, policy studies or management and has been qualitative and descriptive in nature (Rodríguez, Késenne, & Humphreys, 2011).[2] Meanwhile, economic research on participation in sport and its relationship to health is not well developed. Our paper contributes to this relatively unexplored field in economics and uses quantitative, econometric methods (i.e., instrumental variable regression) to draw causal conclusions on the relationships between leisure-time sports activity and health-related outcomes.

The main challenge in disentangling the relationship between sports activity and health-related outcomes is that individuals endogenously decide whether to participate in sports activities or not. Therefore, sports activity is likely correlated with many (unobserved) confounders that can conceal true causalities. For example, health-conscious people with a high level of body awareness may be more active but also tend to get more health screenings and tend to visit the doctor more often. Another confounder is a person's (healthy or unhealthy) lifestyle, for example her nutrition or sleeping behaviour. A healthy lifestyle is likely to be positively correlated with sports activity and negatively correlated with health issues and health care utilization.

To address this self-selection problem, we employ an instrumental variables (IV) strategy and use the geographic availability of sports facilities to predict individuals'

[2] Of course, participation in sport has also been widely examined from a clinical perspective in disciplines like sports medicine, sports psychology or sports training.

level of sports activity. The reasoning behind this strategy is that living close to sports facilities implies easier access to sports infrastructure and reduces the "costs" of doing sports. Therefore, geographic proximity to sports facilities is an ideal instrument because it increases sports activity, and the supply of sports facilities is (at the individual level) exogenous to unobservable factors affecting health and health care utilization.[3] We realize this strategy using representative and geocoded data from the *Swiss Household Panel* (SHP) and the Swiss *Business Census* for the year 2008. The geocoding of the data allows us to pinpoint linear distances between the residence of SHP respondents and all sports facilities obtained from the Swiss *Business Census* with a precision of a few meters and shows that proximity to a large number of sports facilities significantly increases individual sports participation.

Based on this data, we find that doing sports at least once a week significantly reduces the number of doctor visits, overweight and sleeping problems. The magnitudes of these effects are larger in the IV estimations than in non-IV estimations, which seem to be biased toward zero due to reporting errors in sports activity and an omitted variable bias. Furthermore, our IV estimations show that sports activity has no causal effect on the frequency of back problems and headaches, whereas non-IV estimations find sports activity also to reduce these health issues. The non-IV results seem to be negatively biased and to suffer from a reverse causation issue insofar as individuals with back problems and headaches tend to reduce sports activity.

Taken together, the results of the study affirm that leisure-time sport is an important determinant of a healthy community and society and that the availability of sports facilities plays an important role in individuals' decisions to participate in

[3] As residential neighborhoods are not randomly assigned, we cannot completely rule out that unobserved health determinants could influence residential sorting into neighborhoods with few or many sports facilities. Therefore, the validity of the *exogeneity* condition of our IV models is carefully discussed in the paper.

such activities. Our results are useful for estimating the cost-effectiveness of providing sports facilities to encourage individual participation in sports activities as a way of reducing health problems and health care utilization and also support the view that governments should continue subsidizing sports activity in an environment of reduced public resources (Rodríguez et al., 2011, p. 2).

In conclusion, this dissertation illuminates – in addition to the individual contributions of the three papers – the many exciting possibilities which arise from economic analysis of the sports industry and sports data. While this research field has long been regarded as an enjoyable pastime for traditional economists who like sports, the significance of sports in the economy and in society has expanded dramatically in recent years and so has the importance of literature on the economics of sports. With the global sports market now being estimated to achieve revenues of more than US$100 billion each year (PwC, 2011), the analysis of sports has occupied a permanent and established position in economic research. The following three papers contribute to this emerging strand of literature and lay the ground for future research either to benefit the sports industry through economic thinking, theory and methods or to benefit economics through the analysis of human behavior observed in professional sports.

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# A Appendix: Papers included in this dissertation

# A.1
# Managers' external social ties at work: Blessing or curse for the firm?*†

**Abstract**

Existing evidence shows that decision makers' social ties to internal coworkers can lead to reduced firm performance. In this article, we show that decision makers' social ties to external transaction partners can also hurt firm performance. Specifically, we use 34 years of data from the National Basketball Association and study the relationship between a team's winning percentage and its use of players that the manager acquired through social ties to former employers in the industry. We find that teams with "tie-hired-players" underperform teams without tie-hired-players by 5 percent. This effect is large enough to change the composition of teams that qualify for the playoffs. Importantly, we show that adverse selection of managers and teams into the use of tie-hiring procedures cannot fully explain this finding. Additional evidence suggests instead that managers deliberately trade-off private, tie-related benefits against team performance.

**JEL Classification**: D82; M51; Z13
**Keywords**: social relationships; social capital; principal–agent relationship; worker allocation; basketball

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# 1 Introduction

A person's social relations are a key influence factor for her attitudes, preferences, and (economic) decision-making. When searching for a job, for example, individuals have been found to frequently rely on information and resources from their social contacts (Montgomery, 1991; Bewley, 1999; Ioannides & Loury, 2004; Jackson, 2006). In the workplace, *newly* formed social ties to others *within* the firm have been found to affect employee productivity and overall firm performance (Bandiera, Barankay, & Rasul, 2005, 2008, 2009, 2010).

This paper documents field evidence on whether and how employees' *history* of social relations and experiences *outside* the firm influences firm-level decision-making and overall firm performance. We focus on a prominent form of historical, external social relationships: pre-existing, strong social ties to colleagues at a former employer in the same industry. Such ties are potentially very influential for firm-level decisions, as they create opportunities for on-going business transactions (e.g., resource acquisitions). However, the question whether tie-influenced transactions pose a blessing or a curse for the firm remains unresolved. On the one hand, external ties to others in the industry may help firm performance, as they provide superior access to relevant market information. On the other hand, it is reasonable to expect that external social ties can harm firm performance if they interfere with employees' optimal selection of transaction partners.[1]

[1] Bandiera et al. (2009) and Beaman and Magruder (2012) argue that social networks create network-based incentives, which lead to a form of social transfer between network contacts. This explains why individuals prefer to recommend their less able family members (instead of more able weak ties) as workers to firms. Similarly, Lawler and Yoon (1998) argue that interactions through social ties lead to greater positive emotions than interactions with strangers. Such private benefits for decision makers may distort their decision-making on behalf of the firm, and may lead to an excessive reduction in the universe of potential transaction partners, which causes a suboptimal match of resources and firms. Note that this idea is essentially an agency argument.

To determine the overall performance effect of tie-influenced transactions we construct a novel data set from an unusual but interesting industry: the National Basketball Association (NBA).[2] Specifically, we use the complete event history of the NBA in its current form (since 1977) and combine a team's record of player acquisitions and sporting performance with data on the working history of its key decision maker: the general manager. Our empirical focus lies on the performance effect of player acquisitions that the general manager[3] makes from his former employers in the NBA. Therefore, we test the null hypothesis that teams with "tie-hired-players" show identical sporting performances as teams without tie-hired-players.

Four characteristics make our unusual setting ideal to study the overall performance effect of managers' external, social ties. First, each team employs only one manager at a time who is ultimately responsible for the team's most important transactions: player acquisitions. Second, we have industry-wide information on each manager's complete working history, and the identity of his former colleagues (i.e., team owners and head coaches). In each season, this allows us to identify each manager's set of active, strong social ties to other teams in the NBA. Third, we observe the number of game appearances for each player in the industry, which allows us to measure the relative importance of tie-hired-players in team production. Finally, we

[2]There exists a growing literature that uses sports data sets to study general economic and organizational phenomena, because they provide statistics that "are much more detailed and accurate than typical microdata samples" (Kahn, 2000, p. 75). Examples include Pfeffer and Davis-Blake (1986), Walker and Wooders (2001), Berman, Down, and Hill (2002), Chiappori, Levitt, and Groseclose (2002), Barden and Mitchell (2007), Moliterno and Wiersema (2007), Holcomb, Holmes Jr, and Connelly (2009), Aime, Johnson, Ridge, and Hill (2010), Price and Wolfers (2010), Pope and Schweitzer (2011), Berger and Pope (2011), Kocher, Lenz, and Sutter (2012), Massey and Thaler (2013), and Bartling, Brandes, and Schunk (2014).

[3]In the remainder of this paper, we use the simple term "manager" to refer to a team's general manager.

observe an objective measure of team performance: the team's sporting success in the regular season.[4]

Our empirical analysis shows that the effect of tie-hired-players on team performance is negative. Based on a simple mean comparison, we find that teams with tie-hired-players underperform teams without tie-hired-players by a substantial 11 percent. Subsequent regression analyses reveal that this difference in winning percentages stems from teams' use of tie-hired-players on the court and not from (unobserved) quality differences of teams and managers: controlling for manager and team fixed-effects, a team's budget, and other observable characteristics, the average tie-hired-player reduces team performance by about 5 percent. Importantly, we show that the negative performance effect of tie-hired-players is robust across two additional social tie definitions that include up to 190 tie-hired-players.

In an extended analysis, we address the underlying mechanism for this finding and show that tie-hired-players reduce team performance only if they have been acquired in the presence of low monitoring incentives for team owners. Our estimation approach builds on different streams of psychological research (e.g., Schoorman, 1988; Shepherd, Wiklund, & Haynie, 2009) suggesting that monitoring incentives should be lower for an owner who personally hired a manager than for an owner who "inherited" a manager from the previous owner. Information on manager turnover in the NBA supports the idea that new owners engage in stronger monitoring: within one year of an ownership change, 48 percent of pre-existing managers are replaced. Overall, the results of our

[4] A small existing literature in finance and strategic management relies on investor reactions to decision announcements as a"jury verdict" to measure the performance effect of tie-influenced decisions (e.g., Fracassi & Tate, 2012; Tian, Haleblian, & Rajagopalan, 2011; Ishii & Xuan, 2014). However, the announcement of, e.g., merger decisions may cause substantial disagreement regarding the performance effect among investors (which are also known to exhibit a number of systematic valuation biases). This evaluation problem disappears in our research setting: at the end of a game, there can be no doubt which team won.

study suggest that managers deliberately use their external social ties to pursue goals other than team performance maximization.[5]

A unique feature of the institutional environment of our data allows us to address potential concerns about endogeneity bias as a source for our finding. That is, players may either be hired in the off-season period between two seasons, or after the beginning of a new season. To avoid any feedback from team performance at the beginning of the season on subsequent hiring decisions, we conduct another analysis, in which we focus only on a team's use of off-season tie-hired-players. Based on this approach, we still find a negative performance effect of tie-hired-players, and that this effect stems from tie-hired-players that the manager acquired under weak monitoring. Even when we acknowledge that off-season tie-hired-players may be influenced by a team's performance in the previous season, we find that the performance effect of tie-hired-players is negative and depends on whether they have been acquired under weak or strong monitoring by the owner. Overall, we show that adverse selection of teams and managers into the use of tie-hiring procedures cannot fully explain our findings.

While the setting of this analysis is unusual, the results of our study have fairly broad implications. Several studies in the management and economics literature reveal that employees' external social ties influence their decision-making on behalf of the firm, for example, in connection with hiring (Fernandez & Weinberg, 1997; Williamson & Cable, 2003) financing (Shane & Cable, 2002), or investing (Cohen, Frazzini, & Malloy, 2008).[6] Anecdotal evidence also seems to suggest that firms often seek well-connected employees. Our industry-wide analysis shows the hidden costs of such hiring practices, and reveals the novel finding that network-based incentives can lead

[5] Importantly, we do not find evidence in our data that ownership changes reflect a previous reduction in team performance: team winning percentage in the year before the arrival of a new owner (46.3%) is virtually identical to the team's average winning percentage in all previous years under the original owner (46.8%).

[6] However, these studies do not address the performance effect of tie-influenced decisions for the firm.

to discrimination of external transaction partners.[7] We also show that firms can counterbalance this form of discrimination, if they are willing to incur additional costs (e.g., in the form of extended monitoring). This second finding extends, and confirms the insights of a recent, small economic literature that shows how incentive contracts reduce workers' favoritism toward socially connected others (Bandiera et al., 2009; Beaman & Magruder, 2012; Beaman et al., 2013).

We structure this paper as follows. In the next Section, we provide a brief background on player acquisitions and managers in the NBA. In Section 3, we present our research hypothesis, and theoretical framework. In Section 4, we present our estimation approach to determine the effect of tie-hired-players on team performance. In Section 5, we present our empirical results. In Section 6, we conclude.

## 2 Background information

To follow the analysis in this paper, it is important to have some background regarding the NBA, its labor market and the role of NBA managers. In this section, we therefore briefly discuss the nature of player acquisitions in the NBA and two key aspects of managers in the NBA: their stereotypical profile and their outside options on the labor market.

Since its merger with the American Basketball Association (ABA) in 1976, the NBA has been the only major professional basketball league in Northern America. The (combined) league initially had 22 teams in 1976/77 and has expanded since, and

[7]Few empirical studies address the negative performance effects of external social ties. However, the findings of these studies differ from ours, as they only show negative performance effects when decision makers bring their social contacts inside the firm (e.g., through job recommendations (Beaman & Magruder, 2012; Beaman, Keleher, & Magruder, 2013), through mergers (Ishii & Xuan, 2014) or as supervisors to reduce monitoring (Fracassi & Tate, 2012)). As we discuss further in Section 4.2, a manager's social ties in our study do not relate to the hired player, but to the coach or owner (or both) of the player's current team. Accordingly, the manager's social ties still remain outside the boundaries of his team after a player transaction.

as of 2011, the NBA consists of 30 teams in two conferences. Each team plays 82 games over the course of a season, before the eight best teams in each conference proceed into the playoffs to determine the league champion. To increase their performance, teams compete for the most talented players on a restricted labor market.

There are two important features of player acquisitions in the NBA that distinguish them from the hiring decisions of firms in other industries. First, a team can only acquire new players from three different types of sources. These are: other teams inside the NBA, other teams outside the NBA, and the annual player draft. In the annual draft, teams are allowed to select upcoming college, high school or international players from a pool of new, young players (so-called rookies). Acquisitions from other NBA teams are by far the most popular choice of managers and account for 67 percent of all player-hiring decisions followed by the draft (21%) and transactions with other teams outside the NBA (12%). Thus, we can treat the NBA as a nearly closed system of extraordinarily talented workers (who generally spend their entire careers within this industry).

Second, there are two specific ways for a team to acquire players from other teams inside the NBA. First, a manager can sign a player whose contract with another team has expired as a "free-agent" by outbidding all other interested parties. This transaction type accounts for 52.1 percent of all between-team transactions.[8] Second, managers can trade their players with on-going contracts for players with on-going contracts from other teams. In this case, a single trade may involve multiple (>2) transaction partners, each potentially trading more than one player. This transaction

[8]The exact procedure behind such free-agent signings differs slightly: in 74 percent of such signings, the player received a long-term contract, in 22 percent, the player received a short-term contract (a so-called 10-day-contract), and in 4 percent the player was acquired by means of the expansion draft (which provides newly created teams the opportunity to recruit players from a specific set of "unprotected" players from existing teams).

type accounts for the other half (47.9%) of all player transactions between NBA teams. Note that this transaction type does not require the consent of the players involved.

In each team, the responsibility for player acquisitions rests exclusively with the team's (general) manager who has been hired by the team owner to act on his behalf. As we are ultimately interested in the consequences of the managers' decision-making, it is illuminating to look at these individuals more closely. We construct the stereotypical manager profile[9] by looking at a manager's average characteristics at the beginning of a new contract spell throughout our sample period (1977/78 until 2010/11). Based on this approach and the 146 active managers in this period, we can characterize the stereotypical (newly hired) manager to be 46 years old, with slightly more than two years of previous experience as a manager (where he generated an average winning percentage of 0.488), and holding up to three previous positions as manager in the NBA. 30.1 percent of the managers had a previous history as coach and 40.5 percent of the managers had a previous history as player at the beginning of a new spell (resulting in a combined average of 50.3 percent of managers with a previous history as a player or coach (or both) in the NBA).

Regarding managers' possibilities of getting re-hired at another NBA team after the end of a work spell, we observe that around 31 percent take up another job as a manager in the NBA. However, as far as managers' outside options on the labor market are concerned, many managers also re-appear in the NBA in other jobs after the end of their manager career. While the exact job positions can be manifold, a considerable 41 percent of these former managers take up one of the following four

[9] We thank an anonymous referee for this suggestion.

positions with an NBA team: (assistant) coaches (14%), (vice) presidents (10%), advisor (9%) or scouts (8%).[10]

# 3 Research hypothesis

We assume that a team owner hires a manager to maximize team performance by acquiring the best available basketball players (subject to budget constraints). The players that the manager acquires from other NBA teams come either from former employers (we refer to such players as tie-hired-players), or from other teams (non-tie-hired-players). We test the null hypothesis that teams with tie-hired-players show identical sporting performances as teams without tie-hired-players.

**Hypothesis 1:** The sporting success of a team does not depend on its use of tie-hired-players instead of non-tie-hired-players.

In contrast to this null hypothesis, social capital (e.g., Adler & Kwon, 2002) and agency theory (e.g., Eisenhardt, 1989) predict that tie-hired-players can affect team performance. Although both theories suggest that managers use their social ties deliberately to economize on search costs, they disagree on the associated performance effect: social capital theory predicts a positive performance effect of tie-hired-players, whereas agency theory predicts a negative performance effect. In the following, we discuss each of these theories.

[10]These numbers are based on an analysis for 109 inactive managers with website entries on Wikipedia.com. Note that the first two of these four positions are frequently rumored to be even better paid than manager positions: some websites claim that the average manager salary was USD 1.5 million in 2009, and thus somewhat lower than the USD 2.0 million for coaches. Similarly for team presidents, there is word that the average salary is comparable to that of Fortune 500 CEOs and thus even higher (around USD 10 million in 2012). While these numbers partly lack official confirmation, they suggest that the disciplinary power of the labor market for managerial decision-making may be quite limited.

In the NBA, information is an important element in properly matching players to teams and positions. However, the search for better players and information comes at substantial costs for teams, which calls for mechanisms to reduce such costs. In this regard, a manager's social ties may prove valuable to his team for several reasons. Uzzi (1996), for example, notes that decision makers can reduce the high level of uncertainty in hiring decisions through fine-grained information transfer in social tie relationships. Similarly, Jackson (2010) argues that social networks allow for the mitigation of substantial search frictions, as they enable the communication of critical information to firms regarding the potential fit of workers. The use of social ties further reduces search costs, as decision makers are able to use trusted social contacts that are already in place and need not invest in constructing new ones (Granovetter, 2005). A manager who wishes to acquire the best available players in the market can thus use his strong social ties to former employers as an instrument to achieve this goal with substantially lower search costs for his team. Specifically, he can select an acquisition source through his social ties, as the relational characteristics of social ties allow for a more reliable information exchange based on trust and closeness (Moran, 2005).[11]

It is important to see that this reasoning can make the use of social ties beneficial during conceptually different acquisition procedures such as player trades and free-agent signings. That is, both acquisition procedures provide opportunities for interactions and information exchange through social ties. In trades, the direct transaction partner is the player's current team. In free-agent signings, the player's current

[11] A manager's external ties to other teams constitute "bridging ties" (in the sense of McEvily & Zaheer, 1999), because they connect his team "to sources of information and opportunities that are not available from other network contacts" (p. 1136). Intuitively, this view implies that social ties to players' current employers provide more precise information about their playing quality, than any other form of intra-industry social ties. In contrast to Granovetter (1973), McEvily and Zaheer (1999) argue that such bridging ties are not always weak ties. Indeed, the degree of knowledge sharing between organizational units has been shown to increase with tie-strength (Tortoriello, Reagans, & McEvily, 2012).

team does not form the direct transaction partner (because the team no longer holds property rights over him) but can be contacted for up-to-date information about the free-agent, and his availability.

However, agency theory may also have some explanatory power in the context of tie-influenced player acquisitions in the NBA. That is, the owner-manager relationship exhibits all of the factors necessary to cause substantial agency costs. First, the owner and manager are linked by a principal–agent relationship in which the manager has been hired to act on the behalf of the owner. Second, the manager has substantially greater expert knowledge in professional basketball than the owner, which gives the manager an informational advantage: between 1977/78 and 2010/11, only 3 percent of team owners could build on a career history as player or coach in professional basketball, while a (slight) majority of 53 percent of managers could do so. Accordingly, managers can be assumed to have a substantially higher specific knowledge (most of which can be assumed to be tacit knowledge from their game experiences) about "what it takes" for a team to succeed in the NBA. Third, the owner is unable to judge the quality of a manager's search effort, as a player's fit into a team cannot be directly inferred from his performance statistics with other teams. Instead, the manager must expend substantial search effort to improve the fit. As the marginal benefit of this search effort is unobservable, the manager has the opportunity to use social ties to pursue his self-interest instead of the team owner's interest. We now provide a theoretical justification for why managers' and team owners' self-interests may not be perfectly aligned.

Researchers in the corporate governance literature have long acknowledged that the residual claims of owners are unlimited in time whereas the employment contracts of managers have limited durations by definition. As a consequence, owners have incentives to pay attention to the entire future stream of payoffs (cash, utility, prestige etc.) generated by their firm, while managers will only value payoffs yielded during their limited tenure with the firm (see Jensen & Smith, 1985, p. 11). As a consequence, managers systematically place lower value on payoffs that occur beyond their limited time horizon (see also Jensen & Meckling, 1979; Furubotn & Pejovich, 1973), which can distort their decision-making to the disadvantage of the owner.

In the context of the NBA, we find that this line of reasoning might indeed have some explanatory bite for the decision-making of managers: while managers stay, on average, for five years with a team, the corresponding tenure for owners is almost twelve years, and thus significantly larger ($t = 7.18$, $p < 0.001$). Moreover, owners have tradable residual claims, which allow them to capitalize on future payoffs. Accordingly, a manager will base his behavior much more on the involved search costs (which he incurs today) than on the decisions' long-term implications (which he bears only for a limited time). This reasoning stands in sharp contrast to the owner who bears the complete long-term implications (e.g., in terms of reduced future team value) of the manager's decisions. In connection with the labor market's limited disciplinary power for managers (see Section 2), suboptimal hiring decisions (from the owner's perspective) by the manager become a real possibility.[12]

Two examples for managers' self-interest maximization to the disadvantage of team owners are choices characterized by inefficiently low effort levels and the selection of inefficient transaction partners that create private benefits for the manager. In the first case, the use of social ties helps to reduce disutility from search efforts, as social relations form a salient selection criterion for prechoice activities. Such activities re-

[12]In spite of the idea that managers pursue self-interests that differ from those of owners, team owners might prefer some managers to others. That is, the owner perceives manager A to be better than manager B if A's decisions lead to smaller agency costs for the owner than B's decisions.

duce personal workload, as they reduce the number of choice alternatives that need to be evaluated in the decision process.[13] Similarly, Levin and Cross (2004) acknowledge that managers may simply approach socially tied others for convenience. This can cause better-suited players in the market to be neglected, as they are currently under contract with unrelated teams. In the second case, managers often derive additional, private utility from interactions with socially connected others. Specifically, such interactions can produce positive emotions such as feelings of pleasure and enjoyment (Lawler & Yoon, 1998; Bandiera et al., 2010) and can lead to a form of "consumption on the job" for managers. Therefore, such network-based incentives can distort the manager's cost-benefit evaluation of a transaction partner, leading again to an inefficient focus on socially tied teams in player acquisitions.[14] Again, this effect may influence managers' decision-making for player trades, and free-agent signings, alike.

We want to stress that both types of self-interest maximization can occur *although* managers have strong incentives to do well with their teams. Specifically, we acknowledge that a manager's future career depends on how well he does with his current team. However, this does not imply that the manager is never willing to engage in suboptimal hiring decisions. Instead, it suggests that suboptimal hiring decisions can occur whenever the increase in expected utility for the manager (as previously described) outweighs his expected disutility from a (slight) reduction in team performance. Importantly, our data suggest that managers can get away with reduced team performance much more easily than coaches: while a coach's appointment ends, on

[13] See the discussion in Beach (1993).

[14] See Bandiera et al. (2009) and Beaman and Magruder (2012) for analytical models that can be adopted to reflect the decision problem for managers in the NBA. Intuitively speaking, the manager has two sources of utility: a (sporting-) performance-dependent bonus if he hires a "good" player, and a social transfer (monetary or non-monetary) from transactions with socially tied others. If, all else equal, the social transfer is sufficiently high, the manager may be willing to forego the performance-based utility component, and hire a "mediocre" player (i.e., a player with suboptimal match) through his social ties.

average, after 2.79 years, managers remain with a team for about five years ($t = 5.64$, $p < 0.001$).

# 4 Estimation approach

## 4.1 Data

We construct a new data set with all 908 team-year observations in the period from 1977/78 until 2010/11. For each season, our data set includes information on each team's regular-season winning percentage and roster characteristics (such as payroll, total number of players on the roster, total game appearances of players, and new players on the roster). We combine this data with the complete transaction history between all teams. We obtained this information from *Sports Reference LLC*, a professional company that specializes in the collection and publication of sports data.

## 4.2 Identification of manager social ties and tie-hired-players

We focus on a prominent type of managers' social relationships to identify their set of external social ties: the social ties to colleagues at a former employer (i.e., another NBA team).[15] Such ties are potentially very influential for managers' decision-making, as managers frequently acquire players from other teams inside the NBA. Therefore, it is reasonable to expect that managers who have started a new employment relationship continue to interact with their former employers on the market for player talent. Our

[15]Similarly, McEvily, Jaffee, and Tortoriello (2012) use co-working histories of lawyers to study the effect of employees' external social ties on firm growth. However, our focus on personal ties to other teams implies that we only include between-team player acquisitions in our analysis. While this procedure may seem restrictive at first glance, there is good reason to exclude drafted players. Camerer and Weber (1999), for example, show that top drafted players in the NBA play excessive minutes (relative to their performance). That is, teams often "overuse" their top draft picks, which can lead to negative performance effects. Similarly, teams may expose substantial biases that lead to financial overvaluation of top picks (see Massey and Thaler (2013) for evidence in the National Football League (NFL)).

data support this idea: managers are 32 percent more likely to acquire players from socially tied teams than from unrelated teams.

We identify a manager's active social tie to another team from two requirements. First, he must previously have worked for that team (as a manager). Second, the current owner or head coach (or both) of that team continue to be his former colleagues. This second requirement stems from the observation that a manager's working history with another team may inappropriately reflect a *social* tie if none of his former colleagues continue to work for that team.[16] To operationalize, on a seasonal level, the set of active social ties to other teams for each of the 146 managers in our sample, we collect his full employment history (including work spells before 1977) and combine it with the full employment and ownership histories of head coaches and team owners, respectively.

The following example helps to clarify our identification approach: In 2004, John Nash was the manager of the Portland Trailblazers. At this point, Nash had an employment history with the Philadelphia 76ers and the Washington Wizards, and hence these were two potential candidates for his set of external, strong social ties. During Nash's time in Washington (1991–1996), Abe Pollin had been the owner of the Wizards, and he remained the owner in 2004. Thus, Nash had an active social tie to Washington in 2004.[17] However, we do not observe an active social tie to Philadelphia, as the coaches and owner he had worked with at Philadelphia during 1987–1990 had

[16]As we show in the Appendix, our results are robust to the use of two extended social tie measures. The first measure also includes a manager's history as a coach with former teams. The second measure allows for the possibility that a manager maintains ties to all his previous employers, irrespective of whether his colleagues on the coach, manager or owner level are still with those teams (meaning that we drop the second requirement of our original identification approach).

[17]This is a very representative example for the origin of social ties in our sample leading to tie-influenced player acquisitions. Specifically, 84.7 percent of all our tie-hired-players arrived through purely owner-related ties, while another 13.9 percent of our tie-hired-players arrived through ties that include both, the owner and the coach. Only a mere 1.4 percent (only one case) of our tie-hired-players arrived through purely coach-related ties. Therefore, we are unable to model different effects for tie-hired-players that arrived through pure coach-ties and those that arrived through pure owner-ties. We leave this important aspect for future research.

already left before 2004. Note that our procedure gives rise to non-reciprocal social ties between managers and teams: for example, the manager of Washington in 2004, Wes Unseld, did not have a social tie to Portland, as he had never worked for that team before.

To classify players into the groups of tie-hired-players and non-tie-hired-players, we use the complete record of all player acquisition decisions in our sample period. We identify a player as a tie-hired-player if a manager's social tie was involved in the player's acquisition and if it is the player's first season with the new team. We focus on a player's first year for two reasons. First, teams might drop players who performed poorly in their first season, which is why using multiple years would create a survivorship bias in our estimates. In fact, only 38 percent of all tie-hired-players in our sample stay with their team for more than one season. Second, players acquire tacit knowledge and assimilate over time. Thus, a player's performance in his first season with a team promises to be a better quality measure of the hiring decision than his performance in subsequent seasons.[18] Based on this approach, and depending on the restrictiveness of the social tie definition (see Appendix), we classify between 72 and 190 players in our data set as tie-hired-players.

An example of a tie-hired-player is when the New Jersey Nets acquired Eduardo Najera from the Denver Nuggets on July 16, 2008. Before that, New Jersey's manager, Kiki Vandeweghe, had worked with George Karl (the 2008 head coach of the Nuggets) and Stan Kroenke (the 2008 owner of the Nuggets) at Denver. To re-emphasize an important point: the decisive criterion for a player to be classified as a tie-hired-player is not that the manager gained first-hand information about this player during

[18] As we show in Section 5.3, however, our results are robust to an alternative analysis in which a tie-hired-player keeps his status as a tie-hired-player during all seasons of his initial contract with the new team.

previous employment spells, but that the manager acquired the player from a team to which he had an active social tie at the time of the acquisition.

Table 1 provides summary statistics for the most important variables in our data set. In Panel A, we show statistics on the team level. While teams had tie-hired-players in only 6 percent ($N = 53$) of our team-year observations, the use of tie-hired-players (if present) is quite substantial: on average, all tie-hired-players on a team appear in 50 games for their teams.[19] Note from Table 1 that payroll information is unavailable for nine seasons (1977/78–1984/85, and 1989/90) during our sample period, which leads to a substantially lower number of observations ($N = 700$).

In Panel B, we show statistics on the individual manager level. Of particular interest is a manager's potential for tie-hiring decisions. We construct this number as follows: in each season, a manager has as many opportunities for tie-hiring decisions, as he has active ties to other teams. By summing up these seasonal opportunities over his career years, we obtain his total potential for tie-hiring decisions. On average, this potential is 1.39 leading to 0.50 tie-hiring decisions over the career. While these numbers are quite low, they reflect on the small number of managers who ever worked for more than one team. Therefore, Panel C provides the same statistics for the subsample of managers who ever had any ties. For each of these managers, the statistics reflect only years with active social ties. We can see that these managers account for 22 percent of all managers in our sample, had a potential for tie-hiring decisions of 6.36, and made on average two tie-hiring decisions throughout those years, which amounts to 6.7 percent of all their hiring decisions. Note that there exists substantial heterogeneity among managers, as this share is as high as 50 percent for some of them.

[19] A closer examination of our data also shows that approximately 50 percent of all NBA teams used tie-hired-players on the court in at least one season.

**Table 1**
Summary statistics.

| Variables | Mean | Std.dev. | Min. | Max. | $N$ |
|---|---|---|---|---|---|
| Panel A: team level | | | | | |
| Team winning percentage | 0.50 | 0.15 | 0.134 | 0.878 | 908 |
| Team uses tie-hired-player (dummy) | 0.06 | 0.23 | 0 | 1 | 908 |
| Games played by tie-hired-players | 49.53 | 43.28 | 0 | 194 | 53 |
| Games played by all team players | 819.58 | 64.69 | 471 | 944 | 908 |
| Players used within season | 16.19 | 2.44 | 11 | 27 | 908 |
| Payroll (in mio $) | 36.85 | 23.17 | 2.91 | 101.37 | 700 |
| Panel B: manager level (all managers) | | | | | |
| Potential for tie-hiring decisions (over career) | 1.397 | 3.736 | 0 | 20 | 141 |
| Number of tie-hiring decisions (over career) | 0.511 | 1.329 | 0 | 8 | 141 |
| Number of hiring decisions (over career) | 40.830 | 35.298 | 3 | 186 | 141 |
| Share of tie-hiring decisions | 0.011 | 0.045 | 0 | 0.50 | 141 |
| Career length (years) | 6.440 | 5.626 | 1 | 25 | 141 |
| Panel C: manager level (managers with social ties in years with social ties) | | | | | |
| Potential for tie-hiring decisions | 6.355 | 5.707 | 1 | 20 | 31 |
| Number of tie-hiring decisions | 2.065 | 2.097 | 0 | 8 | 31 |
| Number of hiring decisions | 38.839 | 29.243 | 4 | 124 | 31 |
| Share of tie-hiring decisions | 0.067 | 0.088 | 0 | 0.50 | 31 |
| Career years with social ties | 5.613 | 4.652 | 1 | 17 | 31 |

**Notes:** With the exception of payroll (unavailable for 1977/78–1984/85, and in 1989/90), displayed statistics are for the 1977/78–2010/11 seasons.

## 4.3 Methodology

To analyze the effect of tie-hired-players on team performance, we regress a team's sporting performance on the number of game appearances by tie-hired-players, payroll, number of players used (to account for bad injury luck), and a team's number of games played by all players. We always use the exact number of game appearances such that, for example, the use of two tie-hired-players in one game leads to two more game appearances by tie-hired-players. Importantly, we also include team and manager fixed-effects to account for the performance effect of unobserved team and manager quality, respectively.[20] By controlling for a team's payroll, the coefficient on game appearances by tie-hired-players (*THP-games*) indicates whether a team with tie-hired-players over- or underperforms relative to what could be expected from the

[20]From time to time, teams relocate and re-appear in the league under a new name. However, the league treats these teams as a continuous legal entity, independent of the team name and host city. Similar to Barden and Mitchell (2007) for Major League Baseball, we adopt the league's perspective on the identification of team-units (e.g., the Oklahoma City Thunder and the Seattle Supersonics are the same team in our data).

market valuation of its player talent in a specific season. To make payrolls comparable across seasons, we use inflation-adjusted payrolls (1986 = 100) in all our estimations. Note, however, that the inclusion of the payroll variable comes at a cost, as this information is not available for each season in our sample.

Our approach closely follows previous work by Szymanski (2000) and models a team's logarithmic winning percentage as a function of team-level variables (relative to their league averages in a season):

$$\begin{aligned} \log(\textit{win-pct})_{ts} \;=\; & \beta_0 + \beta_1 \cdot \overline{\textit{THP-games}}_{ts} + \beta_2 \cdot \overline{\textit{payroll}}_{ts} + \\ & \beta_3 \cdot \overline{\textit{players-used}}_{ts} + \beta_4 \cdot \overline{\textit{team-games}}_{ts} + \alpha_t + \alpha_m + \epsilon_{ts}, \end{aligned} \tag{1}$$

where the subscripts $t$, $m$ and $s$ denote teams, managers and seasons, and where $(\bar{\cdot})$ denotes the difference between a variable and its league average in season $s$. The dependent variable $\log(\textit{win-pct})$ is the (logarithmic) regular season winning percentage of team $t$ in season $s$.[21]

The coefficient of interest is $\beta_1$ and measures the effect of tie-hired-players on team performance. By our inclusion of a team's players' total number of games played in Eq. (1), $\beta_1$ answers the following question: what is the performance effect of using a tie-hired-player in one more regular season game, holding the overall number of player-game appearances for the team constant. That is, $\beta_1$ measures the performance effect of the substitution of a tie-hired-player for a non-tie-hired-player on the team, as increasing the number of games played by tie-hired-players corresponds to a reduction

[21] An alternative empirical approach would have been to adopt an event study design, in which team performance in matches before the hiring decision is compared to team performance in matches after the hiring decision. We decided not to adopt such an empirical design, because many hirings occur in the "off-season" period. That is, in many cases, there exists a substantial time gap between matches before and after the hiring decision, which makes this identification approach less appealing to us.

in the number of games played by non-tie-hired-players. While social capital theory predicts that $\beta_1$ will be positive, agency theory predicts it will be negative.

# 5 Empirical results

## 5.1 Model-free evidence

Before we turn to the estimation results of Eq. (1), we report the results of a model-free analysis of our data. Specifically, we compare winning percentages across the groups of teams that use tie-hired-players on the court and teams that do not.[22] We find that teams with tie-hired-players win 45.2 percent of their regular season games, while teams in the other group win 50.2 percent of their games ($t = 2.31$, $p < 0.05$). This implies that teams with tie-hired-players underperform their competitors without tie-hired-players by 11 percent. This finding therefore provides initial, suggestive evidence against the null hypothesis that a team's use of tie-hired-players does not impact its sporting success.

## 5.2 Regression analysis

Table 2 shows regression estimates for the performance effect of using tie-hired-players on the team instead of other players. In Model (M1), we only introduce team fixed-effects in the analysis, while Models (M2) and (M3) incorporate our other controls and manager fixed-effects, respectively. In contrast to the null hypothesis, all models reveal that tie-hired-players reduce team performance. We emphasize that the negative performance effect of tie-hired-players cannot simply reflect adverse selection

[22]To make teams more comparable, we exclude eleven team-year observations in which a team did not acquire any new players from other teams inside the NBA. However, our results are robust to the inclusion of these observations.

**Table 2**
The effect of the use of tie-hired-players (THP) on team performance.

| Variables | OLS M1 (1978–2011) | OLS M2 (1986–2011) | OLS M3 (1986–2011) |
|---|---|---|---|
| Games played by THP | –0.0024*** | –0.0024** | –0.0017** |
| | (0.0008) | (0.0009) | (0.0008) |
| Games played by all team players | 0.0010** | 0.0003 | 0.0004 |
| | (0.0004) | (0.0004) | (0.0005) |
| Payroll (in $10^6$) | – | 0.0212*** | 0.0179*** |
| | | (0.0055) | (0.0047) |
| Players used within season | – | –0.0461*** | –0.0429*** |
| | | (0.0057) | (0.0072) |
| Team fixed-effects | Yes | Yes | Yes |
| Manager fixed-effects | No | No | Yes |
| Observations | 897 | 694 | 694 |

**Notes:** The dependent variable is a team's (logarithmic) regular season winning percentage. All independent variables are measured relative to their league averages in a season. Robust standard errors that have been adjusted for clustering at the team level are given in parentheses. All estimations also included a constant (not reported). *, **, and *** denote statistical significance at the 10%, 5%, and 1% levels, respectively.

of managers into the use of social ties as an acquisition practice, because manager fixed-effects in Model (M3) serve as control for each manager's time-invariant "quality type".

We find that the effect of tie-hired-players on team performance is large: on average, each tie-hired-player plays approximately 36.5 games per season. According to our estimates from Model (M3), the on-court use of one such tie-hired-player results in a 5.2 percent reduction in the regular season winning percentage. For the 50 teams that barely made the playoffs in our sample by claiming the 8th spot in their conferences, this would have resulted in 2.1 fewer regular season wins. In 64 percent of the seasons in our sample, this difference in wins would have been sufficient to drive the team ranked 8th in its conference to 9th place (thereby missing the playoffs). This finding implies that the impact of social ties on the hiring behavior of managers can be crucial for making the playoffs.

## 5.3 Alternative explanations for the negative performance effect

While our main finding is perfectly in line with the predictions of agency theory, other explanations may come to mind. For example, in spite of the negative short-term performance, tie-hired-players might be good long-term investments. Another possibility could be that managers use their social ties to realize non-sporting benefits for the team. If this was true, our focus on sporting performance might give a downward biased view on the benefits of tie-hired-players.

According to the view that tie-hired-players are good long-term investments, managers might use their social ties to acquire players, so called "diamonds-in-the-rough" that have great upside potential, but need some time to develop. Such hiring decisions are beneficial to the team, if their negative performance effect in the first year is more than offset by positive performance effects over the following years of their contract. To address this possibility, we perform another analysis in which we re-classify a player as a tie-hired-player if his current team acquired him via a social tie, and if he is still under his initial contract with that team. Note that this measure includes all tie-hired-players as in our main specification but also includes tie-hired-players that have already been with the team for more than one season. As Table 3, Panel A shows, the associated coefficient on the game appearances of such "long-term tie-hired-players" remains negative and statistically significant at the 10 percent level. Overall, this finding contradicts the notion that the use of social ties in player acquisitions leads to superior team performance in the longer-run.

Alternatively, it could be that managers use their social ties in acquisition decisions as a means to create non-sporting benefits for the team. To address this possibility,

**Table 3**
Alternative explanations for the negative performance effect.

| Panel A: THP as long-term investments? (THP re-definition) | | |
|---|---|---|
| *Variables* | OLS (1986–2011) | |
| Games played by THP (complete contract) | –0.0015* | (0.0008) |
| Observations | 694 | |
| **Panel B: good value for money? (salaries of free-agents)** | | |
| *Variables* | OLS (1986–2011) | |
| Social tie (dummy) | 0.0488 | (0.1738) |
| Observations | 835 | |

**Notes:** In Panel A, the dependent variable is a team's (logarithmic) regular season winning percentage. The estimation included all control variables as Model (M3) in Table 2. Robust standard errors that have been adjusted for clustering at the team level are given in parentheses. In Panel B, the dependent variable is a player's (logarithmic, inflation-adjusted) salary. The estimation included controls for player age, experience, past performance and salary, as well as fixed-effects for position, and team. Robust standard errors that have been adjusted for clustering on the player level are given in parentheses. All estimations also included a constant (not reported). *, **, and *** denote statistical significance at the 10%, 5%, and 1% levels, respectively.

we consider a key non-sporting benefit: a player's reduced monetary wage cost. To determine the effect of manager social ties on player wage costs, we focus on players that were acquired as free-agents. The reason for this restriction is that for traded players, the acquiring team continues to pay the same salary that the player used to receive from his previous team. Instead, the salary of a free-agent can be freely negotiated between the player and his new team. In Table 3, Panel B shows the results from a Mincer-type wage regression model, in which we model a free-agent's (logarithmic, inflation-adjusted) salary payment as a function of his age, experience, past performance and salary, as well as fixed-effects for position, and team. Importantly, we also include a variable that indicates whether the player was acquired from a socially tied team of the manager. As our results show, we do not find any significant influence from a social tie being involved on the players' salary level.

The lack of an empirical relationship between social ties and a player's salary level helps to rule out two additional alternative explanations that might come to mind.[23]

[23] We are grateful to an anonymous referee for bringing these explanations to our attention.

First, social ties might give rise to exaggerated perceptions of player ability as a form of cognitive bias. Second, managers might view acquisitions through social ties as less risky. While the underlying mechanisms differ, both explanations imply that managers should have a higher willingness to pay for free-agents that they acquire through social ties (conditional on observable characteristics) than for free-agents that they acquire from unrelated teams. As already mentioned, however, the result in Table 3, Panel B does not provide evidence for this prediction.

## 5.4 Extended analysis: monitoring incentives and the performance effect

We now aim to test more directly whether the use of social ties in hiring decisions represents deliberate opportunistic behavior by managers.[24] Our test is based on the idea that if managers maximize utility taking into account private benefits that stem from interactions with former employers, we expect that this type of opportunistic behavior should be more pronounced when monitoring by the team owner is weak. As a consequence, tie-hired-players should be most detrimental to team performance if they were acquired under weak monitoring.

To test this prediction, we assume that a manager faces weaker monitoring if he has personally been hired by his owner than if he has been hired by a previous team owner. For example, the literature on emotional costs of failure asserts, “greater negative emotions are generated when one’s own decision “causes” the onset of the negative

[24] Alternatively, it could be that managers wish to benefit the team with tie-hired-players but mistakenly make poor decisions for the team. For example, previous work has highlighted that the external social ties of decision makers can harm firm performance due to poor decision-making in response to a heightened sense of trust between socially tied actors, familiarity bias, or social conformity and groupthink (e.g., Ishii & Xuan, 2014). Similarly, social capital theorists have long acknowledged that decision makers can become overly embedded in social networks, which reduces opportunities for collaboration (Granovetter, 1985), because network contacts feel obliged to assist each other (rather than members outside the social network).

outcome rather than when others make that decision" (Shepherd et al., 2009). This observation implies that an owner who personally hired a (bad) manager faces greater negative emotional costs from replacing this manager. In anticipation of these costs, the owner might deliberately reduce the "detection probability" of a bad manager by reducing his monitoring activity. In a similar vein, the literature on the escalation of commitment has shown that supervisors change their employee performance evaluation upwards when they were directly included in the hiring decision and agreed with the selection of the candidate (Schoorman, 1988). Our data provide support for this idea: as new owners collect more and more information over time, the share of pre-installed managers that have been replaced increases from 48 percent in the first year to 58, and 63 percent after two and three years, respectively.

Therefore, we re-estimate Eq. (1) but distinguish between tie-hired-players that were acquired by managers under weak monitoring, and tie-hired-players that were acquired by managers under strong monitoring. Note that the difference between weak and strong monitoring stems from the order of individuals' arrivals at the team: under weak monitoring, the manager arrived *after* the current owner, while under strong monitoring, the manager arrived *before* the current owner. Table 4 displays the associated estimation results. In line with our prediction, we find a statistically significant, negative performance effect of tie-hired-players that were acquired under weak monitoring. In contrast, we do not find a statistically significant effect from tie-hired-players that were acquired under strong monitoring. An $F$-test supports the impression that the coefficients for tie-hired-players across the two monitoring regimes are significantly different ($F = 4.58$, $p < 0.05$). Our data shows that this finding does not simply reflect reverse causality between ownership changes and team performance: in the year before the arrival of a new owner, a team wins about 46 percent of its

**Table 4**
The effect of the use of tie-hired-players (THP): indeed an agency conflict?

| Variables | OLS (1986–2011) |
|---|---|
| Games played by THP | –0.0020** |
| (acquired under weak monitoring) | (0.0008) |
| Games played by THP | –0.0002 |
| (acquired under strong monitoring) | (0.0006) |
| Games played by all team players | 0.0004 |
| | (0.0005) |
| Payroll (in $10^6$) | 0.0180*** |
| | (0.0047) |
| Players used within season | –0.0433*** |
| | (0.0073) |
| Team fixed-effects | Yes |
| Manager fixed-effects | Yes |
| Observations | 694 |

**Notes:** The dependent variable is a team's (logarithmic) regular season winning percentage. All independent variables are measured relative to their league averages in a season. Robust standard errors that have been adjusted for clustering at the team level are given in parentheses. All estimations also included a constant (not reported). *, **, and *** denote statistical significance at the 10%, 5%, and 1% levels, respectively.

games, which is the same as its average winning percentage in all previous years under the original owner.

Overall, these additional findings make it unlikely that behavioral biases (such as familiarity bias, excessive trust, or distorted perceptions of player ability and acquisition risks) or overembeddedness of managers are the predominant mechanism behind the negative performance effect. Instead, we take our findings as evidence that, in line with the prediction of agency theory, managers trade off private benefits against team performance. As expected, managers are less likely to engage in such moral hazard behavior if properly monitored by the owner.

## 5.5 Exploiting institutional feature: addressing endogeneity

As with all non-experimental studies there exist reasons to be concerned about endogeneity bias as a source for our findings. For example, McDonald and Westphal (2003) show that decision makers have a greater tendency to rely on their social ties when firm performance is already low. This poses a potential adverse selection problem for our analysis, because teams frequently acquire players after the beginning of a new season. Specifically, it could be that the negative performance effect of tie-hired-players reflects exclusively on the poor performance that teams already showed before they acquired these players. In this subsection we use two different specifications to address this potential concern.

In the first specification, we exploit an institutional feature of the NBA, namely that seasons in the NBA are divided into a foregoing off-season period between June and October (when team preparation occurs) and a playing period beginning in November. Teams usually acquire their players during the off-season but are allowed to make roster adjustments during the playing period. In the following, we focus only on off-season tie-hired-players and exclude all tie-hired-players who were acquired after the beginning of the playing season. This chronological separation of hiring decisions and the performance generating mechanism (the games) implies that off-season tie-hired-players cannot reflect low performance early in the season. Technically speaking, the timely separation implies that the number of off-season tie-hired-players is predetermined in the team performance regression. In Table 5, Model (E1) displays estimation results when we re-estimate Eq. (1) by only considering games played by off-season tie-hired-players. While the reduction in the number of tie-hired-players leads to a reduction in statistical significance, we still find a negative performance effect that is statistically significant at the 10 percent level. Importantly, Model (E2) shows that this negative performance effect stems exclusively from tie-hired-players that were acquired under weak monitoring. Again, an $F$-test shows that the coefficients for tie-hired-players across the two monitoring regimes are significantly different ($F = 6.99$, $p < 0.05$).

**Table 5**
Tests for endogeneity: off-season THP and team performance.

| Variables | OLS E1 (1986–2011) | OLS E2 (1986–2011) | OLS E3 (1986–2011) | OLS E4 (1986–2011) |
|---|---|---|---|---|
| Games played by off-season THP | –0.0022* | – | –0.0012 | – |
| | (0.0011) | | (0.0009) | |
| Games played by off-season THP (acquired under weak monitoring) | – | –0.0027** | – | –0.0018** |
| | | (0.0011) | | (0.0008) |
| Games played by off-season THP (acquired under strong monitoring) | – | 0.0004 | – | 0.0012* |
| | | (0.0010) | | (0.0007) |
| Games played by all team players | 0.0004 | 0.0003 | 0.0007* | 0.0007* |
| | (0.0005) | (0.0005) | (0.0004) | (0.0004) |
| Payroll (in $10^6$) | 0.0180*** | 0.0151*** | 0.0096** | 0.0097** |
| | (0.0047) | (0.0046) | (0.0037) | (0.0037) |
| Players used within season | –0.0433*** | –0.0410*** | –0.0417*** | –0.0422*** |
| | (0.0072) | (0.0073) | (0.0068) | (0.0069) |
| Lagged team winning percentage ($s-1$) | No | No | Yes | Yes |
| Team fixed-effects | Yes | Yes | Yes | Yes |
| Manager fixed-effects | Yes | Yes | Yes | Yes |
| Observations | 694 | 694 | 689 | 689 |

**Notes:** Displayed are estimation results for extended versions of Eq. (1). The dependent variable is a team's (logarithmic) regular season winning percentage. All independent variables are measured relative to their league averages in a season. Robust standard errors that have been adjusted for clustering at the team level are given in parentheses. All estimations also included a constant (not reported). The difference in observations between models E1/E2 and E3/E4 relates to the exclusion of five expansion teams' first-year observations (for which a lagged winning percentage is not available). *, **, and *** denote statistical significance at the 10%, 5%, and 1% levels, respectively.

In the second specification, we acknowledge that off-season tie-hired-players may partly reflect on the team's sporting performance in the previous season (Moliterno & Wiersema, 2007). This poses a problem for our estimation whenever a team's sporting performance is considerably lower than its long-term average (which is reflected in the team fixed-effects). Therefore, we also estimate two models, in which we control for a team's lagged winning percentage from the previous season ($s-1$). In Table 5, Model (E3) reveals that this variable reduces the statistical significance of tie-hired-players' game appearances. While we still find a negative performance effect in this model, the effect becomes marginally insignificant ($p = 0.176$). However, Model (E4) provides a simple explanation for this reduction in statistical significance: when controlling for a team's lagged winning percentage, the negative coefficient on tie-hired-players under weak monitoring is still statistically significant at the 5 percent level, but reduces in size (in absolute terms). In contrast, the positive coefficient on tie-hired-players under strong monitoring increases relative to Model (E2) and even becomes statistically significant. Accordingly, an $F$-test strongly rejects the coefficient equality for tie-hired-players across monitoring regimes ($F = 14.18$, $p < 0.01$). Taken together, these findings imply that the pooled measure in (E3) must lose statistical significance relative to our findings in Model (E1). We emphasize that while the positive, statistically significant effect from tie-hired-players under strong monitoring seems to support the prediction from social capital theory that the use of social ties can benefit the firm, this finding depends critically on the monitoring incentives for the owner. In addition, some caution seems to be in order as the coefficient is only marginally significant ($p = 0.099$).

Overall, the evidence from these two additional specifications that pose much less scope for endogeneity bias confirms that the reason behind the negative performance effect of tie-hired-players lies in the lack of sufficiently strong monitoring from team owners.

# 6 Conclusion

In this paper, we provide industry-wide evidence on the overall performance effect of employees' use of external strong social ties to others outside the firm. We focus on external social ties to a prominent group of firm outsiders: colleagues at a former employer in the same industry. The fact that such ties are usually strong ties, which persist beyond shared co-working experiences makes them potentially very influential for firm-level decisions. An important question for firms is therefore whether ties to former employers should be expected to interfere with the selection of transaction partners in decision-making on behalf of the firm.

We add to the existing knowledge by providing an analysis of a unique, naturally occurring panel field data set that provides a rare opportunity to determine the relevance of employees' external social ties for firm-level decision-making in the field. Based on the complete transaction history between all teams in the National Basketball Association in its current form (34 years), we show that a manager's external, social ties to his past (employers) can harm team performance in the present. The effect is large: controlling for a team's budget and other characteristics, the average tie-hired-player reduces team performance by about 5 percent. We also find that the negative performance effect is entirely driven by managers under team owners with low monitoring incentives. These findings lend support to the idea that – in the absence of appropriate performance incentives – network-based incentives can sometimes undermine firm-level objectives.

# Appendix

In this appendix, we consider two extensions of our social tie measure. In the first extension, we include a manager's history as manager and coach, as it is not unusual for a manager in the NBA to have formerly worked as a coach with other teams. In the second extension, we acknowledge the possibility that a manager may maintain social ties to his former employers via connections to former colleagues on other levels than coach, manager or owner level.

While we believe that a manager's ties to former colleagues at these latter levels are most valuable for player acquisition decisions, we emphasize that this extension provides additional credibility to our findings as it considerably extends the number of tie-hired-players in our sample from $N = 72$ in the main text to $N = 100$ (Extension 1) and $N = 190$ (Extension 2). However, in constructing this second extension, we face the considerable challenge that complete information on each employee's working history in the NBA is unavailable. Therefore, we assume that a manager has a social tie to another team if he has previously worked as either a manager or coach for that team, irrespective of whether his former colleagues on the coach, manager, or owner level are still with that team. Note that this second extended measure is potentially much more noisy than the measure in our main specification, which is due to the unobservability of social ties to colleagues on other levels. Table A.1 provides summary statistics for both extended measures.

Table A.2 presents estimation results when we replicate our main regression analyses with the extended sets of tie-hired-players, and shows that our key findings are robust to the use of both extensions: besides a statistically significant, negative performance effect of tie-hired-players (Models MA1 and MA3), we also continue to find that only tie-hired-players that were acquired under weak monitoring reduce team performance (Models MA2 and MA4).

**Table A.1**
Summary statistics: extended social tie measures.

| Variables | Mean | Std.dev. | Min. | Max. | *N* |
|---|---|---|---|---|---|
| Panel A: active ties (including manager's history as coach) | | | | | |
| Team level | | | | | |
| Team uses tie-hired-player (dummy) | 0.082 | 0.274 | 0 | 1 | 908 |
| Games played by tie-hired-players | 47.892 | 44.422 | 0 | 194 | 74 |
| Manager level (all managers) | | | | | |
| Potential for tie-hiring decisions (over career) | 2.085 | 4.252 | 0 | 20 | 141 |
| Number of tie-hiring decisions (over career) | 0.709 | 1.641 | 0 | 10 | 141 |
| Number of hiring decisions (over career) | 40.830 | 35.298 | 3 | 186 | 141 |
| Share of tie-hiring decisions | 0.017 | 0.052 | 0 | 0.50 | 141 |
| Career length (years) | 6.440 | 5.626 | 1 | 25 | 141 |
| Manager level (managers with social ties in years with social ties) | | | | | |
| Potential for tie-hiring decisions | 6.255 | 5.326 | 1 | 20 | 47 |
| Number of tie-hiring decisions | 2.000 | 2.303 | 0 | 10 | 47 |
| Number of hiring decisions | 37.851 | 28.532 | 4 | 124 | 47 |
| Share of tie-hiring decisions | 0.060 | 0.084 | 0 | 0.50 | 47 |
| Career years with social ties | 5.362 | 4.245 | 1 | 17 | 47 |
| Panel B: active ties and non-active ties (including manager's history as coach) | | | | | |
| Team level | | | | | |
| Team uses tie-hired-player (dummy) | 0.139 | 0.346 | 0 | 1 | 908 |
| Games played by tie-hired-players | 48.690 | 43.577 | 0 | 194 | 126 |
| Manager level (all managers) | | | | | |
| Potential for tie-hiring decisions (over career) | 3.773 | 6.725 | 0 | 39 | 141 |
| Number of tie-hiring decisions (over career) | 1.348 | 2.826 | 0 | 20 | 141 |
| Number of hiring decisions (over career) | 40.830 | 35.298 | 3 | 186 | 141 |
| Share of tie-hiring decisions | 0.031 | 0.064 | 0 | 0.50 | 141 |
| Career length (years) | 6.440 | 5.626 | 1 | 25 | 141 |
| Manager level (managers with social ties in years with social ties) | | | | | |
| Potential for tie-hiring decisions | 8.185 | 7.890 | 1 | 39 | 65 |
| Number of tie-hiring decisions | 2.892 | 3.597 | 0 | 20 | 65 |
| Number of hiring decisions | 40.569 | 30.889 | 4 | 139 | 65 |
| Share of tie-hiring decisions | 0.074 | 0.081 | 0 | 0.50 | 65 |
| Career years with social ties | 6.277 | 6.061 | 1 | 22 | 65 |

**Notes:** Displayed statistics are for the 1977/78–2010/11 seasons.

**Table A.2**
The performance effect of tie-hired-players (THP): extended social tie measures.

| Variables | Extension 1 | | Extension 2 | |
|---|---|---|---|---|
| | OLS<br>MA1 | OLS<br>MA2 | OLS<br>MA3 | OLS<br>MA4 |
| Games played by THP | –0.0013**<br>(0.0005) | – | –0.0009*<br>(0.0005) | – |
| Games played by THP<br>(acquired under weak monitoring) | – | –0.0015***<br>(0.0004) | – | –0.0012***<br>(0.0004) |
| Games played by THP<br>(acquired under strong monitoring) | – | 0.00001<br>(0.0008) | – | 0.0003<br>(0.0009) |
| Games played by all team players | 0.0003<br>(0.0005) | 0.0003<br>(0.0005) | 0.0003<br>(0.0005) | 0.0003<br>(0.0005) |
| Payroll (in $10^6$) | 0.0180***<br>(0.0047) | 0.0181***<br>(0.0047) | 0.0181***<br>(0.0047) | 0.0181***<br>(0.0047) |
| Players used within season | –0.0427***<br>(0.0073) | –0.0431***<br>(0.0073) | –0.0427***<br>(0.0074) | –0.0425***<br>(0.0074) |
| Team fixed-effects | Yes | Yes | Yes | Yes |
| Manager fixed-effects | Yes | Yes | Yes | Yes |
| Observations | 694 | 694 | 694 | 694 |

**Notes:** The displayed estimation results follow our core estimation model (Eq. (1)). The dependent variable is a team's (logarithmic) regular season winning percentage. All independent variables are measured relative to their league averages in a season. Robust standard errors that have been adjusted for clustering at the team level are given in parentheses. All estimations also included a constant (not reported). *, **, and *** denote statistical significance at the 10%, 5%, and 1% levels, respectively.

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# A.2
# Tie-transfers and player performance in professional soccer*

**Abstract**

In this paper, we document empirical evidence on how a past working relationship between coach and player affects the quality of the transfer decisions made by professional soccer clubs. Using employment records to identify past ties between coaches and players, we analyse 19 years of transfer decisions from clubs in the German Bundesliga and show that players acquired through tie-transfers outperform players acquired through market-transfers by 3 percent. However, we also show that not all tie-transfers are equally desirable. Extended analyses reveal that tie-transfers where a new coach brings a player straight from his previous club result in an underperformance of the newly acquired players of almost 8 percent. Taken together, our findings imply that referrals from coaches can both improve and worsen the quality of clubs' transfer decisions and that the contextual factors are crucial for an effective use of such referral-hiring practices.

**JEL Classification**: L83; D82; M51
**Keywords**: soccer; transfer; player performance; coach; ties

*This paper has been written jointly with Raphael Flepp and Egon Franck.

# 1 Introduction

The global transfer market in professional soccer has become big business in the last few decades, as clubs have exhibited ever greater efforts to improve their competitive position through player transfers. Nowadays, clubs pay gigantic transfer fees to sign players that are in running contracts with other clubs. For the year 2013, the FIFA Transfer Matching System recorded total transfer fees of €2.8 billion for international transfers alone (FIFA TMS, 2014). Related figures from UEFA show that some top European clubs, such as Real Madrid, Barcelona and Manchester United, each have a current squad that has cost them more than €250 million in transfer fees (UEFA, 2014). Given these enormous investments, the optimization of every transfer is crucial for professional soccer clubs.

While plenty of information on soccer players is publicly available to all the clubs in the market, including historical club affiliations, match appearances, and a wide range of performance statistics, decision-making in the transfer market involves many poorly known factors, such as game intelligence, playing style and suitability for the team's playing style, personality and ability to mesh with team-mates and coaches (see the discussion in Weinberg, 2014). To gain a competitive advantage over other clubs in the competition, clubs have to incorporate *hidden* information about players into their decision-making in the transfer market.

A historically evolved concept for a club to do so is to rely on its coach[1] as an information source. The coach works together with the players on a daily basis and thereby gathers first-hand information about their capabilities. As both coaches and players

[1] In this paper, the term "coach" is used to refer to the employee of a club who holds responsibility for devising team tactics and strategies, line-up selection, and the coaching and training of players. This terminology is common in most European soccer leagues and in the major sports leagues in the United States. However, well-known exceptions are the English soccer leagues where the employee of a club with responsibility for first-team playing matters is usually called "manager" (Dobson & Goddard, 2011).

move among different clubs during the course of their professional careers, coaches stockpile information about many players in the market. Accordingly, ties between coaches and players created by a past working relationship constitute a valuable resource for clubs which can be incorporated into transfer decisions. An illustrative example is FC Bayern Munich's signing of Thiago Alcántara in summer 2013, Bayern's coach Pep Guardiola having previously worked with the player at FC Barcelona.

Arguably, clubs should be able to realize benefits from using such coach–player ties in transfer decisions, as they allow for the mitigation of substantial search frictions in the recruitment of players (see the discussion in Jackson, 2010). However, a beneficial use of coach–player ties strongly depends on the ability of coaches to screen their player network effectively. Existing evidence on job referrals indicates that this ability cannot be taken for granted: creating short-term jobs in a field experiment, Beaman and Magruder (2012) have found that many employees are not able to make good referrals even when they are properly incentivized to refer high-ability workers. Beaman and Magruder concluded that not all employees can screen effectively for their employers because they simply do not have a good understanding of which network members will perform better.

In this paper, we document the first empirical evidence on how ties between coaches and players affect the quality of transfer decisions made by professional soccer clubs. To do so, we construct a novel data set on transfers in the German Bundesliga from the seasons 1995/96 to 2013/2014. First, we compare publicly available information on the employment records of coaches and players to identify all transfers where coach and player have previously worked together. Such "tie-transfers" comprise 6.8 percent of all transfers in the given time period. Second, to address the quality of transfer decisions, we extend our data set with external expert evaluations on the performance of newly acquired players.[2] This novel data set enables us to investigate whether players acquired through tie-transfers outperform players acquired through ordinary "market-transfers".

The empirical analysis of 2,313 transfer decisions in our sample reveals that the effect of tie-transfers on the performance of newly acquired players is positive, overall. Based on a simple mean comparison, we find that players acquired through tie-transfers outperform players acquired through market-transfers by 3.9 percent. Subsequent regression analyses reveal that this difference stems from the coach–player tie to a large extent and not from quality differences of players, clubs or coaches. Controlling for the transfer fee, club fixed-effects, and coach fixed-effects, the performance benefit of tie-transfers is 3.3 percent.

In extended analyses, we investigate whether the effect of tie-transfers depends on more detailed contextual factors. We do so for two reasons: first, research on social networks indicates that tie-benefits can diminish at an alarming rate because the related information is often short-lived and fades quickly (Soda, Usai, & Zaheer, 2004; Burt, 2002). If this view is true in our setting, ties where coach and player have worked together more recently should yield higher quality transfer decisions than those from longer ago. Second, several potential benefits from tie-transfers critically depend on how well the coach knows the players and working environment at his current club (e.g., how well a player fits into the playing style of a team or how well a player meshes with new team-mates). Therefore, tie-transfers involving newly arrived coaches should be less effective, as they can only realize those benefits that stem from

[2] In particular, we draw on expert evaluations from the German soccer magazine *Kicker* which are well-established in economic research on professional soccer (see e.g., Bryson, Frick, & Simmons, 2013; Baumann, Friehe, & Wedow, 2011; Franck & Nüesch, 2010).

player capabilities largely independent of the specific club context (e.g., personality attributes, game intelligence).

Based on a distinction between new and established coaches and a distinction between recent and past ties, we present extended estimations for four subgroups of tie-transfers. Our extended estimations reveal that tie-transfers critically depend on a coach that is already well established at a club. While players acquired through tie-transfers involving an established coach consistently outperform players acquired through market-transfers, tie-transfers involving a new coach are less successful. With a new coach and a past tie, the positive effect of tie-transfers becomes insignificant and with a new coach and a recent tie, the effect of tie-transfers even becomes negative. We emphasize that these differences between the subgroups cannot simply reflect initial problems of newly arrived coaches, because we control for the working experience of a coach at the current club in all our estimations.

That tie-transfers involving new coaches and recent ties should yield poorer performance is puzzling at first sight, because it seems unlikely that a coach would deliberately use his tie to a player to end up with an inferior transfer decision. However, an emerging strand of empirical literature indicates that personal ties between individuals can lead to flawed decision-making (see e.g., Ishii & Xuan, 2014; Fracassi & Tate, 2012; Bandiera, Barankay, & Rasul, 2009; Brandes, Brechot, & Franck, 2015). Most notably, recent evidence from financial economics shows that personal ties between firm directors in mergers can lead to lower value creation. It is argued that directors' tie-influenced decision-making is sometimes flawed in response to familiarity biases, a heightened sense of trust, or social conformity and groupthink (Ishii & Xuan, 2014).

The observed underperformance of players acquired through tie-transfers in the context of new coaches and recent ties may indicate similar behavioral biases. While coaches desire influence on transfer decisions to achieve sporting success for their clubs', and ultimately their own, interests, their referrals can turn out to be poor. Starting at a new club could tempt coaches to target players they have recently worked with because they prefer familiar players around them. Moreover, a heightened sense of trust could lead to a more favorable judgement of players with which coaches have recently worked. The combination of both behaviors can provide a reasonable explanation why tie-transfers lead to a negative effect on the performance of newly acquired players under certain conditions.

We structure this paper as follows. In Section 2, we provide a brief background on player transfers in professional soccer. In Section 3, we outline our research hypotheses. Section 4 presents the data and method. Section 5 includes the empirical analysis, and Section 6 concludes.

## 2 Background information on player transfers

To follow the analysis in this paper, it is important to have some background on the peculiarities of player transfers in professional soccer and on the involvement of coaches in clubs' transfer decisions. In this section, we therefore briefly discuss basic regulations of the transfer market and the recruitment process of professional soccer clubs.

A player transfer in professional soccer refers to the movement of a player's registration from one club to another and is widely regulated by the world governing body of soccer, the *Fédération Internationale de Football Association* (FIFA).[3] Among other things, the regulations specify transfer windows in which players are allowed to move

[3]While international transfers are under full responsibility of the FIFA regulations, associations at the national level can issue a defined range of country-specific regulations for domestic transfers (FIFA, 2012, p. 7).

and different transfer types for moving from one club to another (FIFA, 2012). In terms of the transfer windows, there are two fixed periods during a season. The first transfer window opens right after the end of a season and closes before the start of the next season. This period may not exceed 12 weeks. The second transfer window occurs in the middle of the season and may not exceed four weeks. Looking at Europe's "big five" soccer leagues[4], transfer activities are highly concentrated in the first transfer window, when more than 80 percent of transfer spendings occur (UEFA, 2012).

In terms of the transfer types for moving from one club to another, three different types can be identified (FIFA TMS, 2014). First, a club can hire a player as a "free-agent" if his contract with the releasing club has expired. This transfer type does not require a transfer agreement between the acquiring club and the releasing club.[5] Second, a club can "buy" a player who is in a running contract with the releasing club. Such transfers are based on the agreement of all parties, as the player moves into a new employment contract with the acquiring club. Normally, this transfer type includes a transfer fee paid by the acquiring club to the releasing club. Third, a club can transfer a player for a defined time-period in a "loan" transfer while remaining in a running contract with the former club.[6] In this transfer type, the employment contract with the acquiring club is only of temporary nature and the loan arrangement may include a lending fee.

[4]Europe's "big five" soccer leagues include the the English Premier League, the German Bundesliga, the Spanish Primera Division, the Italian Serie A, and the French Ligue 1. These leagues are responsible for about 65 percent of all global transfer spendings and thereby account for almost two-thirds of the overall market size (FIFA TMS, 2014).

[5]Prior to 1995, clubs in professional soccer in Europe were allowed to collect transfer fees for players with expiring contracts. However, these transfer fees were eliminated by the Bosman-Ruling of the European Court of Justice in 1995 (see e.g., Binder & Findlay, 2012; Frick, 2009). Because our analysis starts as recently as the season 1995/96, we do not present a further discussion of this regulation change.

[6]Since a player moves back to his former club after an ended loan arrangement, "return from loan" is an additional (fourth) transfer type (FIFA TMS, 2014, p. 50). However, as this transfer type includes an automatic moving back of the player to the former club and no proactive transfer decision, we do not consider such player movement as a transfer in our analysis.

While the aforementioned regulations refer to formal aspects of transfers only, the process by which clubs recruit new players includes many informal aspects. Put as simply as possible, a transfer occurs if a club reasons that a player will benefit the team more than any other available player in the market (under given budget constraints) and if the player values a club's offer higher than offers from all other interested clubs in the market (based on expected future payoff streams).

Clubs identify transfer targets using multiple employees in distinct and complementary roles: scouts, analysts, sporting directors, and coaches can all be involved in making a transfer possible (Poli, 2010). Scouts trek around soccer stadia and pitches nation- and worldwide to gather in-depth information about players that are widely unknown to a club. Analysts comb large databases of player metrics to come up with transfer prospects. Sporting directors[7] have the connections to player agents and clubs and thereby have broader access to the transfer market. Usually, a sporting director also assumes the main responsibility for all negotiations with players and their agents. Finally, coaches can make referrals (as they hold information about many players in the market from working with and playing against them in the past) and have a strong vote in final transfer decisions.[8]

While the specific involvement of most actors in the recruitment process cannot be tracked precisely, the influence of one actor, namely the coach, is manifest in one certain transfer case: decisions on players who are directly known to a coach from previously working together at the same club. Although the level of involvement of

[7] In professional sports, there are several alternative job titles for the role of the "sporting director": e.g., "general manager" in the major sports leagues in the United States and "director of football" in the English soccer leagues.

[8] Of course, the initiator of a transfer may not only be a club proactively attempting to improve the playing quality of the current squad but also a player dissatisfied with prospects at his current club who seeks better opportunities elsewhere (Carmichael, Forrest, & Simmons, 1999). To do so, players usually engage agents who introduce them to clubs with a view to negotiating employment contracts and who conclude transfer agreements between the clubs if needed (FIFA, 2008). However, unlike to many other aspects of professional soccer, details on the activities of the so-called player agents are not generally publicly available.

coaches in player recruitment can vary from club to club (Dobson & Goddard, 2011), it is almost certain that coaches are at least consulted on the recruitment of players with whom they have worked before.

# 3 Research hypotheses

Ties between coaches and players created by a past working relationship are a valuable resource for decision-making in the transfer market. First, the past working relationship creates a personal tie between a coach and a player that includes informational advantages as well as superior access. Second, such ties between coaches and players emerge as a mere by-product of the co-working experiences and clubs do not have to specifically invest in them. Third, a coach has strong incentives to make his personal ties available in the recruitment process in order to enhance transfer decisions, because the threat of getting fired in response to poor sporting performance is a very powerful disciplinary mechanism in the labor market for soccer coaches (see e.g., D'Addona & Kind, 2014; De Paola & Scoppa, 2012; Barros, Frick, & Passos, 2009). Considered together, ties between coaches and players created by a past working relationship should allow clubs to reduce the unknowns in transfer decisions and to improve the quality of transfer decisions. Therefore, we predict that players acquired through tie-transfers outperform players acquired through market-transfers.

Our prediction on ties between coaches and players created by a past working relationship is closely related to theory on the referral-hiring phenomenon. Following Fernandez, Castilla, and Moore (2000), we see three analytically distinct ways by which clubs can realize benefits from hiring through their coaches' player networks. First, coach–player ties expand a club's recruitment horizon and tap into pools of players who would otherwise not be available. Second, coach–player ties provide informational advantages, as they pass on extra, hard-to-measure information about players and their specific fit into the current team. Third, coach–player ties might ease the transition of a player into a new club and team structure in the post hire period because of the past co-working experience with the coach. All three processes acquire players who will outperform those obtained in market-transfers (where no prior coach–player tie exists).

However, we suggest that the potential of previous coach–player ties to enhance the quality of transfer decisions is likely to vary with contextual factors. First, one can ask whether the time lag between the tie and the transfer affects the benefits of tie-transfers, because compelling evidence from network research indicates that benefits decay quickly. For example, a study on past collaboration in the television production industry found that network benefits derived from "bridging ties" diminish rapidly because the relevance of the information is often short-lived (Soda et al., 2004).[9] Similarly, a study on networks in the banking industry showed that bridging ties tend to fade within two years because of the high opportunity costs of maintaining them (Burt, 2002). This logic suggests that the associated benefits of tie-transfers should be more pronounced with more recent ties than with ties from longer ago.

Second, one can ask whether the benefits of tie-transfers hinge on a coach being already well established at his current club. The high reciprocal interdependence of players in the sport of soccer (see the discussion in Keidell, 1987) suggests that a coach needs detailed understanding of the existing team structure to integrate a player

[9] Bridging ties are ties that connect individuals existing in different social spheres that would not be connected otherwise (see e.g., McEvily, Jaffee, & Tortoriello, 2012; McEvily & Zaheer, 1999). In that sense, a tie between a coach and a player created by a past working relationship constitutes a bridging ties if it provides information that would otherwise not be available to the focal club. McEvily and Zaheer (1999) argue that such bridging ties can be simultaneously nonredundant and trusted.

known from the past successfully. Otherwise, coach–player ties may not unleash their full potential, as the lack of working experience with the current team limits any informational advantages that relate to the fit of a player into the specific team structure. Accordingly, we argue that tie-transfers involving a new coach should be less beneficial than tie-transfers involving a coach that is already established.

The aforementioned arguments can be displayed in a 2x2 matrix with four subgroups of tie-transfers (see Table 1). The four subgroups can be described by stereotypical cases. First, a new coach brings in a player he worked with some time ago. Second, a new coach brings in a player directly from his previous club. Third, an established coach calls a player he worked with some time ago. Fourth, an established coach recalls a player who had left the club recently. We expect that benefits of tie-transfers are least pronounced in the case of a new coach and a past tie, because of the diminished relevance of the information acquired through the tie and the lack of knowledge of the existing team structure (H1). In contrast, we expect that benefits of tie-transfers are most pronounced in the case of an established coach and a recent tie, because of the high relevance of the information acquired through the tie and the availability of knowledge of the existing team structure (H4). For the other two cases, H2 and H3, we expect to observe benefits with strength intermediate between those of H1 and H4.

# 4 Data and method

To test our hypotheses, we have created a novel data set on all player transfers in the German Bundesliga from 1995/96 until 2013/14. For each transfer in this time-period, our data set includes information on the player being transferred, information

**Table 1**
Decomposition of tie-transfers into four subgroups.

| Tie decay | Coach entry: New coach | Coach entry: Established coach |
|---|---|---|
| Past tie | Stereotypical case:<br>New coach brings in<br>a player he worked<br>with some time ago<br><br>(H1: +) | Stereotypical case:<br>Established coach calls<br>a player he worked with<br>some time ago<br><br>(H3: ++) |
| Recent tie | Stereotypical case:<br>New coach brings in<br>a player directly from<br>his previous club<br><br>(H2: ++) | Stereotypical case:<br>Established coach recalls<br>a player who had left the<br>club recently<br><br>(H4: +++) |

on the acquiring and releasing clubs, information on the transfer, and information on the coach of the acquiring club. We have combined this data with a comparison of the complete professional careers of coaches and players involved that allows us to identify all tie-transfers in the given time-period. We obtained all this information from *www.transfermarkt.de*, a website that specializes in the free publication of soccer data. Finally, we have completed our data set by adding expert evaluations from the German soccer magazine *Kicker* as a measure of the individual performance of each newly acquired player.

In the following subsections, we describe how the collected data was used to construct the dependent and key explanatory variables and to test our hypotheses.

## 4.1 Dependent variable

Our dependent variable is the individual performance of the newly acquired player in the season of his transfer. To measure individual player performance, we draw on expert evaluations from the German soccer magazine *Kicker*. Sport experts from *Kicker* consistently evaluate the individual match performance of players using the German school grading scale that varies between 1.0 (excellent) and 6.0 (very bad). Each match is attended by two trained representatives of *Kicker* who are experts for the respective teams and who can use additional TV footage in the grading process (Baumann et al., 2011). Importantly, such expert evaluations can take intra-team spillover effects into account when judging the individual contribution of a player in the team production (Franck & Nüesch, 2010). Therefore, the grading constitutes a "composite index" of a player's performance in a match (Frick, 2007), drawing on both quantitative statistics (e.g., goals scored, touches of the ball, passes) and qualitative behaviour on the pitch (e.g., tactical movement, leadership).

To measure the performance of a newly acquired player on the seasonal level, we use the average grading over all matches in which the player was evaluated.[10] To allow for an easier interpretation, we reverse the original grading so that a grading of 6.0 indicates an excellent performance and a grading of 1.0 indicates a very bad performance.

## 4.2 Key explanatory variables

Our main explanatory variable is a dummy variable that indicates whether the coach and the player involved in a transfer have previously worked together in a coach–

[10] To be graded by *Kicker*'s experts, a player must have played for at least 30 minutes in a match. Therefore, matches in which a player was substituted on very late or substituted off very early are not considered in the performance measure. This restriction also creates a minimum requirement for a transfer decision to be included in the sample of our study. If a newly acquired player played no matches (e.g., because of a long-lasting injury) or played for less than 30 minutes per match, we were not able to include the transfer decisions because the newly acquired player was not covered by *Kicker*'s expert evaluation.

player relationship during their professional careers.[11] If yes, a transfer is considered a tie-transfer. If not, a transfer is considered a market-transfer.

Additionally, we decomposed the tie-transfer group into four subgroups of tie-transfers (see 2x2 matrix in Table 1, Section 3). For the operationalization of past and recent ties, we followed the approach of McEvily et al. (2012) in a study on professional ties among lawyers in the legal industry. A tie-transfer is defined as involving a *recent* tie if the lag between the season in which the coach and player last worked together and the season of the transfer is no more than one season. Otherwise, a tie-transfer is defined as involving a *past* tie. Similarly, we operationalized the distinction between new and established coaches. A tie-transfer is defined as involving a *new* coach if the lag between the season in which the coach started work at the current club and the season of the transfer is no more than one season. Otherwise, a tie-transfer is defined as involving an *established* coach.[12]

From all tie-transfers that we identified in the German Bundesliga from 1995/96 until 2013/14, cases with established coaches and past ties are most frequent (42.7%), followed by cases with new coaches and past ties (36.9%) and cases with new coaches and recent ties (17.2%). Cases with established coaches and recent ties contain a very small proportion (3.2%), as only players who are recalled to the same club they recently left can fall into that category. Table A.2 in the Appendix shows a list of all tie-transfer including details on the subgroup categorization.

[11] The "professional career" refers to first teams, reserve teams and more senior youth teams of professional clubs. Working together in amateur clubs and in preadolescent youth teams is not observed in our data.

[12] We emphasize that our operationalization leads to a rather broad measure of recent ties and new coaches. Due to the second transfer window in the middle of a season, a lag of one season may represent a real time lag as long as 18 months. However, to test for the sensitivity of our operationalization, we additionally estimated all our models with two alternative (and more conservative) measures. The additional estimations show that our results remain qualitatively unchanged if we use real time lags of 12 months and 6 months as cut-off points for recent ties and new coaches to build the four tie-transfer subgroups (see Appendix, Table A.1).

## 4.3 Estimation method

To test whether ties between coaches and players created by a past working relationship have an effect on the quality of transfer decisions, we regress the performance of newly acquired players on a tie-transfer dummy and a set of control variables for transfer, player, and coach characteristics. Importantly, we also include club, coach, and season fixed-effects in the estimations to account for performance effects of unobserved club and coach quality and (potential) time-trends in our sample period. We estimate the following model:

$$\textit{performance-grade}_{pcs} = \beta_0 + \beta_1 \, \textit{tie-transfer}_{pcs} + \beta_2 X_{pcs} + \alpha_c + \alpha_{co} + \alpha_s + \varepsilon_{pcs}, \quad (1)$$

where the subscripts $p$, $c$, $co$ and $s$ denote players, clubs, coaches and seasons. Fixed-effects for clubs, coaches and seasons are captured in $\alpha_c$, $\alpha_{co}$, and $\alpha_s$. The dependent variable $\textit{performance-grade}_{pcs}$ is the average grading over all matches of player $p$ with club $c$ in season $s$. The variable $\textit{tie-transfer}_{pcs}$ is a dummy variable that is 1 for tie-transfers and 0 otherwise. The vector of covariates $X_{pcs}$ captures the tactical position of a player[13], player age, coach tenure (at the time of the transfer), transfer type (buy, free-agent, loan) and a dummy for international transfers.

Additionally, $X_{pcs}$ includes the transfer fee paid by the acquiring club to the releasing club in an extended specification. By including the paid transfer fee, we aim to rule out any selection effects in tie-transfers that relate to the raw talent of a player. While other proxies of player talent may come to mind (e.g., historical playing statistics, matches with the national team), we are convinced that the transfer fee paid by

[13] Previous research that draws on *Kicker*'s expert evaluation has shown that goalkeepers and defenders gain systematically higher scores than strikers (Bryson et al., 2013; Baumann et al., 2011). Controlling for the playing position is therefore important to address potential biases in the performance grading stemming from the different tactical responsibilities of different positions (Franck & Nüesch, 2010).

the acquiring club provides a comprehensive proxy of player talent because it reflects the market price of player.[14] However, the inclusion of the transfer fee comes at the cost of a restricted sample, as this measure is only available for transfers that needed a termination agreement on the player's employment contract with the releasing club (i.e., buy transfers).

For extended analyses of the four tie-transfer subgroups that distinguish between new and established coaches and recent and past ties, we re-estimate Eq. (1) with a decomposition of $\textit{tie-transfer}_{pcs}$ into four separate dummy variables. The reference group for the effect of the decomposed dummy variables is, as before, the market-transfer group.

# 5 Empirical analysis

## 5.1 Summary statistics

Table 2 provides summary statistics for the variables in our data set. In Panel A, we show the statistics for the full sample. From the 2,313 transfers, 6.8 percent are identified as tie-transfers. Most of the newly acquired players are midfielders (39.4%), followed by defenders (28.0%) and strikers (27.2%). Goalkeepers only account for a small proportion (5.2%). The average age of newly acquired players is 25.5 years and the average tenure of coaches is 1.9 years at the time (i.e., exact date) of respective transfers. A little under half the sample are international transfers (46.3%). In terms

[14] An alternative empirical approach would be to adopt a fixed-effects model on the unit of the newly acquired player, in which tie-transfers and market-transfers of the same player in different seasons are compared to each other. This would allow to fully account for unobserved (time-invariant) player talent. However, as only 86 of 1,686 players that were involved in the transfers of our sample period appear in multiple transfers with variation in the tie-transfer measure, this approach is less appealing to us. Nevertheless, to account for the fact that some of the newly acquired players in the data set are involved in multiple transfers, the coefficients' standard errors in all our estimations have been adjusted for clustering at the individual player level.

**Table 2**
Summary statistics.

| Variables | Mean | Std.dev. | Min. | Max. |
|---|---|---|---|---|
| Panel A: all transfers ($N$ = 2,313) | | | | |
| Tie-transfer | 0.068 | 0.252 | 0 | 1 |
| Position: Goalkeeper | 0.052 | 0.225 | 0 | 1 |
| Position: Defender | 0.280 | 0.449 | 0 | 1 |
| Position: Midfielder | 0.394 | 0.489 | 0 | 1 |
| Position: Striker | 0.272 | 0.445 | 0 | 1 |
| Age of the newly acquired player | 25.48 | 3.810 | 17 | 39 |
| Performance-grade of the newly acquired player | 3.172 | 0.517 | 1 | 5 |
| Coach tenure (in years) | 1.917 | 2.310 | 0 | 13.7 |
| International transfer | 0.463 | 0.498 | 0 | 1 |
| Transfer type: buy | 0.566 | 0.496 | 0 | 1 |
| Transfer type: free-agent | 0.310 | 0.462 | 0 | 1 |
| Transfer type: loan | 0.124 | 0.330 | 0 | 1 |
| Panel B: buy transfers ($N$ = 1,197)[a] | | | | |
| Tie-transfer | 0.054 | 0.227 | 0 | 1 |
| Position: Goalkeeper | 0.037 | 0.188 | 0 | 1 |
| Position: Defender | 0.267 | 0.443 | 0 | 1 |
| Position: Midfielder | 0.411 | 0.492 | 0 | 1 |
| Position: Striker | 0.285 | 0.452 | 0 | 1 |
| Age of the newly acquired player | 24.95 | 3.538 | 17 | 37 |
| Performance-grade of the newly acquired player | 3.208 | 0.498 | 1 | 5 |
| Coach tenure (in years) | 1.905 | 2.322 | 0 | 13.7 |
| International transfer | 0.505 | 0.500 | 0 | 1 |
| Transfer fee (in million €) | 2.459 | 3.824 | 0.01 | 40.6 |

**Notes:** The data refers to transfers in the German Bundesliga from the seasons 1995/96–2013/14. Data on the performance of newly acquired players includes expert evaluations by the German soccer magazine *Kicker*. All other data is extracted and collected from *www.transfermarkt.de*.
[a]113 out of 1,310 buy transfers in Panel A are not included in Panel B because of unavailable transfer fee data.

of the transfer type, buy transfers are the most popular choice (56.6%), followed by free-agent transfers (31.0%) and loan transfers (12.4%). The average performance grading of the newly acquired players is 3.172.

In Panel B, we show the summary statistics for a reduced subsample that only includes buy transfers. In buy transfers, tie-transfers occur a little less often (5.4%), newly acquired players are a little younger (25.0 years), transfers are more often international (50.5%), and the performance of the newly acquired players is a little higher (3.208). Most notably, Panel B includes the transfer fee paid by the acquiring club to the releasing club.[15] The average transfer fee is €2.46 million (inflation-adjusted with year 2013 = 100). Note that there is substantial variation in paid transfer fees as they range from €10,000 to €40 millions with a standard deviation of €3.82 millions.

## 5.2 Model-free evidence

Before we turn to the estimation results of Eq. (1), we report the results of a model-free analysis of our data. Specifically, we graphically compare the performance across the groups of players acquired through tie-transfers and players acquired through market-transfers (see Figure 1). We find that players acquired through tie-transfers have a mean performance grade of 3.29, while players acquired through market-transfers have a mean performance grade of 3.16, and that this difference of 3.9 percent is statistically significant ($t = 2.86$, $p < 0.01$). This implies that players acquired through tie-transfers outperform players acquired through market-transfers and provides initial, suggestive evidence that tie-transfers can enhance the quality of transfer decisions.

## 5.3 Regression analysis

Table 3 shows regression estimates of the effect of tie-transfers on the performance grade of newly acquired players. Model M1 incorporates control variables for the tactical position of a player, the age of a player, coach tenure, the transfer type, as well as a dummy for international transfers and club, coach, and season fixed-effects. In Model M2, we introduce the transfer fee paid by the acquiring club to the releasing

[15] Transfer fees are based on data extracted and collected from *www.transfermarkt.de*. The data includes best estimates where transfer fees are not disclosed by clubs. Whilst the estimates are not exact science, UEFA itself relies on transfer fee data from *www.transfermarkt.de* in their Club Licensing Benchmarking Report, arguing that the data is deemed suitably accurate for comparative purposes (UEFA, 2012, p. 48).

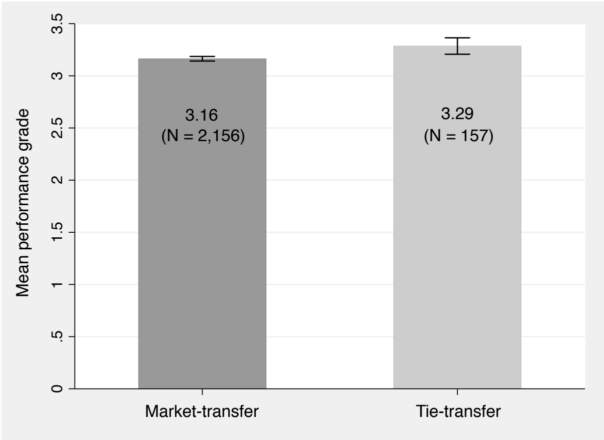


**Figure 1**
Mean performance grade across the groups of players acquired through market-transfers and tie-transfers.

**Notes:** The figure depicts the mean performance grade of newly arrived players as a function of the transfer group by which they were acquired. Market-transfer indicates the group of transfers ($N = 2,156$) in which the coach and the player have not worked together in the past. Tie-transfer indicates the group of transfers in which the coach and the player have worked together in the past ($N = 157$). The difference across both groups of transfers is statistically significant ($t = 2.86$, $p < 0.01$).

club as an additional control variable. As transfer fees are only paid in buy transfers, Model M2 is estimated with a reduced sample of transfers. In Model M3 and M4, we use the log-transformed performance-grade as a dependent variable in order to allow for a non-linear effect from tie-transfers on the performance of newly acquired players.

The results from all models show that tie-transfers increase the performance grade of newly acquired players. For the full sample, we find an absolute increase in performance of 0.076 grading points (Model M1) and a relative increase in performance of 2.5 percent (Model M3). For the buy transfer subsample, we find an absolute increase in performance of 0.103 grading points (Model M2) and a relative increase in performance of 3.3 percent (Model M4). We emphasize that the positive effect cannot reflect selection of better clubs or coaches into the use of tie-transfers, because club and coach fixed-effects serve as a control for their time-invariant "quality type". Moreover, as we include the transfer fee as a control in Models M2 and M4, the positive

**Table 3**
Effect of tie-transfers on the performance of newly acquired players.

| Variables | Performance-grade | | log(Performance-grade) | |
|---|---|---|---|---|
| | M1 (all transfers) | M2 (buy transfers) | M3 (all transfers) | M4 (buy transfers) |
| Tie-transfer | 0.076* | 0.103* | 0.025* | 0.033* |
| | (0.040) | (0.056) | (0.014) | (0.018) |
| Defender | –0.632*** | –0.565*** | –0.179*** | –0.151*** |
| | (0.049) | (0.085) | (0.147) | (0.026) |
| Midfielder | –0.712*** | –0.661*** | –0.205*** | –0.185*** |
| | (0.049) | (0.085) | (0.014) | (0.0.26) |
| Striker | –0.849*** | –0.825*** | –0.255*** | –0.243*** |
| | (0.052) | (0.089) | (0.016) | (0.028) |
| Age | –0.056* | –0.179*** | –0.014 | -0.059*** |
| | (0.031) | (0.046) | (0.011) | (0.016) |
| Age$^2$ | 0.001** | 0.003*** | 0.0003 | 0.001*** |
| | (0.001) | (0.001) | (0.0002) | (0.014) |
| Coach tenure | –0.030*** | –0.029** | –0.010*** | –0.009** |
| | (0.009) | (0.012) | (0.003) | (0.004) |
| International transfer | –0.053** | –0.067** | –0.021*** | –0.025** |
| | (0.021) | (0.030) | (0.007) | (0.010) |
| Free-agent transfer | –0.073*** | – | –0.025*** | – |
| | (0.023) | | (0.008) | |
| Loan transfer | –0.040 | – | –0.016 | – |
| | (0.036) | | (0.013) | |
| Transfer fee (log) | – | 0.095*** | – | 0.034*** |
| | | (0.018) | | (0.007) |
| Club fixed-effects | Yes | Yes | Yes | Yes |
| Coach fixed-effects | Yes | Yes | Yes | Yes |
| Season fixed-effects | Yes | Yes | Yes | Yes |
| Observations | 2,313 | 1,197 | 2,313 | 1,197 |

**Notes:** OLS estimates for Eq. (1) are displayed. The dependent variable is the average grading over all matches played by a newly acquired player in the season of his transfer (non-transformed in Models M1 and M2 and log-transformed in Models M3 and M4). Models M1 and M3 include the full sample. Models M2 and M4 include the reduced sample with only buy transfers. All estimations also included a constant (not reported). Robust standard errors that have been adjusted for clustering at the individual player level are given in parentheses. *, **, and *** denote statistical significance at the 10%, 5%, and 1% levels, respectively.

performance effect cannot simply reflect the selection of players with a higher market price in tie-transfers. Taken together, our findings indicate that, overall, previous coach–player ties have a positive effect on the quality of transfer decisions.

To investigate whether the benefits of tie-transfers depend on the experience of a coach with the current club and the time lag between tie and transfer, we show regression estimates of the effects of the four tie-transfer subgroups in Table 4. The results show that tie-transfers are only beneficial if the coach is well established at the current club. For cases with an established coach and past ties, we observe that newly

**Table 4**
Effect of decomposed tie-transfers on the performance of newly acquired players.

| Variables | Performance-grade | | log(Performance-grade) | |
|---|---|---|---|---|
| | S1 (all transfers) | S2 (buy transfers) | S3 (all transfers) | S4 (buy transfers) |
| *Subgroups of tie-transfers:* | | | | |
| New coach/past tie | 0.089 | 0.136 | 0.031 | 0.041 |
| | (0.059) | (0.089) | (0.020) | (0.028) |
| New coach/recent tie | –0.163* | –0.255** | –0.047 | –0.077** |
| | (0.089) | (0.109) | (0.031) | (0.039) |
| Established coach/past tie | 0.127** | 0.235*** | 0.039* | 0.077*** |
| | (0.060) | (0.079) | (0.021) | (0.024) |
| Established coach/recent tie | 0.551** | 0.438 | 0.165** | 0.131 |
| | (0.269) | (0.336) | (0.079) | (0.094) |
| Defender | –0.632*** | –0.557*** | –0.179*** | –0.149*** |
| | (0.049) | (0.085) | (0.015) | (0.026) |
| Midfielder | –0.714*** | –0.654*** | –0.206 | –0.183*** |
| | (0.048) | (0.085) | (0.014) | (0.026) |
| Striker | –0.853*** | –0.825*** | –0.256*** | –0.243*** |
| | (0.051) | (0.089) | (0.016) | (0.028) |
| Age | –0.057* | –0.184*** | –0.014 | –0.061*** |
| | (0.031) | (0.046) | (0.011) | (0.016) |
| $Age^2$ | 0.001** | 0.004*** | 0.0003 | 0.001*** |
| | (0.001) | (0.001) | (0.0002) | (0.0003) |
| Coach tenure | –0.032*** | –0.032*** | –0.010*** | –0.010** |
| | (0.009) | (0.012) | (0.003) | (0.004) |
| International transfer | –0.050** | –0.065** | –0.020*** | –0.024** |
| | (0.021) | (0.030) | (0.007) | (0.010) |
| Free-agent transfer | –0.072*** | – | –0.025*** | – |
| | (0.022) | | (0.008) | |
| Loan transfer | -0.041 | – | –0.017 | – |
| | (0.036) | | (0.013) | |
| Transfer fee (log) | – | 0.095*** | – | 0.034*** |
| | | (0.018) | | (0.007) |
| Club fixed-effects | Yes | Yes | Yes | Yes |
| Coach fixed-effects | Yes | Yes | Yes | Yes |
| Season fixed-effects | Yes | Yes | Yes | Yes |
| Observations | 2,313 | 1,197 | 2,313 | 1,197 |

**Notes:** OLS estimates for Eq. (1) with a decomposed *tie-transfer*$_{pcs}$ variable are displayed. The dependent variable is the average grading over all seasonal matches of a newly acquired player (non-transformed in Models S1 and S2 and log-transformed in Models S3 and S4). Models S1 and S3 include the full sample. Models S2 and S4 include the reduced sample with only buy transfers. The tie-transfer subgroups are defined as follows: if the seasonal lag between the end of a previous coach–player working relationship and the transfer is no more than one season, the tie is recent; else the tie is past. If the seasonal lag between the entry of a coach at the current club and the transfer is no more than one season, the coach is new; else the coach is established. All estimations also included a constant (not reported). Robust standard errors that have been adjusted for clustering at the individual player level are given in parentheses. *, **, and *** denote statistical significance at the 10%, 5%, and 1% levels, respectively.

acquired players outperform those acquired through market-transfers by 3.9 percent in Model S3 (Model S4: 7.7%). For cases with an established coach and recent ties, the outperformance is even higher at 16.5 percent in Model S3 (Model S4: 13.1%[16]). In contrast, players acquired in tie-transfers involving new coaches do not outperform those acquired in market-transfers, regardless of whether the ties are past or recent.

We find no evidence that benefits of tie-transfers occur only in the short-term because tie-transfers with past ties and established coaches have a significant and positive effect on the performance of newly acquired players. In fact, we find that the least beneficial subgroup of tie-transfers is when ties are actually recent and coaches are new: such newly acquired players perform even worse than newly acquired players from market-transfers, even when we introduce the transfer fee as a control variable (Model S2 and S4). Conditional on the transfer fee paid by a club, players acquired through tie-transfers underperform players acquired through market-transfers by 7.7 percent. Note that this negative effect becomes as high as 14.3 percent if recent ties and new coaches are defined using more conservative cut-off points to operationalize the four tie-transfer subgroups (see Appendix, Table A.1)

# 6 Discussion and conclusion

In this paper, we provide empirical evidence on the effect of previous coach–player ties on the quality of transfer decisions in professional soccer. The first finding of this study is that coach–player ties have a major impact on the selection of players in the transfer market. We observe that 6.8 percent of all transfers in the German Bundesliga include a past working relationship between the coach and the newly acquired player.

[16]Note, that the outperformance of cases with an established coach and recent ties in Model S4 (13.1%) fails to achieve statistical significance ($p = 0.162$) due to the increased standard errors of the estimates that reflect on the very small number of observations in that case.

Second, we show that such tie-transfers enhance the quality of transfer decisions, as players acquired through tie-transfers outperform players acquired through market-transfers by 3 percent. This indicates that coaches, overall, have the ability to screen their player networks effectively in making referrals for transfer decisions.

Third, our findings indicate that tie-transfers are not equally effective in all situations. Players acquired through tie-transfers involving new coaches and recent ties underperform players acquired though market-transfers by almost 8 percent. We emphasize that we observe this negative effect despite the compelling theoretical reasoning that coaches have high incentives to make "good" referrals due to the strong disciplinary power of the labor market for professional soccer coaches (D'Addona & Kind, 2014; De Paola & Scoppa, 2012; Barros et al., 2009). To us, it is most likely that coaches are subject to judgement biases in the context of new entry at a club and recent ties with players. We suggest that new entry at a club may tempt coaches to target familiar but suboptimal players in response to a familiarity bias. In addition, a heightened sense of trust may lead to a more favorable interpretation of players known from recently working together. As a consequence, new coaches could mistakenly make suboptimal referrals on players with which they have worked just before at their prior clubs.[17]

Alternatively, one could try to explain the observed negative effect with network-based incentives that distort coaches' cost-benefit evaluation of transfer decisions (see e.g., Beaman & Magruder, 2012; Bandiera et al., 2009; Brandes et al., 2015). For example, Brandes et al. (2015) argue that managers in professional basketball select suboptimal hirings in order to reduce disutility from search efforts and to derive private utility from interactions with socially connected others. However, we believe

[17]Our explanation closely follows the idea of Ishii and Xuan (2014) who present a similar underlying mechanism for lower value creation in mergers that include personal ties between firm directors.

that this argumentation has low explanatory bite in our case. First, player recruiting is not part of the main job responsibilities of professional soccer coaches and any involvement in transfer decisions results in more and not less search activity for coaches. Therefore, there is not much to reduce in search effort disutility by referring (suboptimal) transfers. Second, the strong disciplinary power of the labor market for coaches in professional soccer and the necessity to be successful on the pitch match after match makes it very unlikely that coaches refer suboptimal transfers in response to some sort of private utility.

The implications of our findings for professional soccer clubs are relatively straightforward. The presented results suggest that previous coach–player ties can be a valuable resource for clubs' transfer market activities. Given the difficulty of observing and evaluating the quality and fit of new players, coach–player ties represent an attractive alternative to better find out the club-specific value of players. A recent illustrative example of a successful tie-transfer is Borussia Moenchengladbach's acquisition of the Brazilian striker Raffael from Dynamo Kyiv (UA) in the summer 2013 (see Table A.2, Nr. 54). The coach of Borussia Moenchengladbach, Lucien Favre, had previously worked with Raffael at Hertha Berlin (2008–2009) and FC Zurich (CH) (2005–2007) and the resulting fit of Raffael with Borussia Moenchengladbach was perfect: Raffael was in the starting line-up in all 34 matches of the season, scored 14 goals and 8 assists and was evaluated (by the *Kicker* experts) as the fourth best striker in the whole league in the season 2013/14.

At the same time, our results also urge clubs to be cautious when a newly hired coach wants to bring players straight from his previous club, because such tie-transfers result on average in poorer transfer decisions. An illustrative example for such a failure tie-transfer is Schalke 04's signing of the Dutch midfielder Orlando Engelaar from

Twente Enschede (NL) in the summer 2008 for a transfer fee of €5.5 million (see Table A.2, Nr. 125). The transfer was evidently initiated by coach Fred Rutten who moved from Twente Enschede to Schalke in the summer 2008, having worked with Engelaar at Twente Enschede from 2006–2008. However, Orlando Engelaar's appointment at Schalke became a disaster. The *Kicker* experts rated Orlando Engelaar as the worst midfielder of the whole league in the season 2008/09 and he left Schalke after just one year with the club.[18]

Of course, our ability to draw definitive conclusions about the effects of ties between coaches and players is limited by several research design considerations of this study. The first limitation stems from the fact that we have data on the performance of the newly acquired players only at the consolidated level of a season. Ideally, we would have data on the level of the individual match that allowed us to investigate intra-seasonal dynamics more precisely (e.g., to better distinguish between informational advantages and ease of transition).

A second limitation is that the admittedly small number of observations in the decomposed subgroups of tie-transfers does not allow us to address within-subgroup variation. Therefore, the application of extended analyses that investigate the subgroup effects more precisely are not feasible. However, we think that especially the underperformance of newly acquired players in the case of new coaches and recent ties would be very interesting for further investigations of the underlying mechanism. A third limitation relates to the that fact that our study focuses solely on newly acquired players. While this focus eliminates concerns of unobserved differences between newly acquired and on-going players, it does not allow investigations of the long-term implications of tie-transfers. Future studies should conduct analyses on the longevity

[18]The somewhat amusing end of the story is that Orlando Engelaar was acquired by PSV Eindoven (NL) in summer 2009 and that Fred Rutten, after dismissal at Schalke in March 2009, was now the coach of PSV Eindhoven.

of the effects of tie-transfers. For example, one could test whether the performance effects persist in the following seasons and whether the length of stay differs between players acquired through tie-transfers and players acquired through market-transfers.

Despite these limitations, we believe that our research makes important contributions by providing new insights into the transfer market in professional soccer. Using the idea that coaches have acquired valuable information on the players they worked with at previous clubs, we demonstrate the benefits of coach referrals for clubs' decision-making in the transfer market but also that not all coach referrals are equally valuable. Depending how much experience a coach has with the current team and the time lag between the previous coach–player relationship and the transfer of the player, we observe positive effects, no significant effects, and sometimes even negative effects of coach referrals on the performance of newly acquired players.

# Appendix

**Table A.1**
Effect of decomposed tie-transfers on the performance of newly acquired players: alternative measures.

| Variables | Performance-grade | | log(Performance-grade) | |
|---|---|---|---|---|
| | (1) (all transfers) | (2) (buy transfers) | (3 ) (all transfers) | (4) (buy transfers) |
| Panel A: using a time period of 12 months to distinguish between new/established coaches and recent/past ties | | | | |
| *Subgroups of tie-transfers:* | | | | |
| New coach/past tie | 0.076 | 0.143 | 0.026 | 0.044 |
| | (0.059) | (0.091) | (0.019) | (0.029) |
| New coach/recent tie | –0.169* | –0.256** | –0.050 | –0.077** |
| | (0.092) | (0.109) | (0.032) | (0.039) |
| Established coach/past tie | 0.128** | 0.200** | 0.040** | 0.065*** |
| | (0.058) | (0.079) | (0.020) | (0.025) |
| Established coach/recent tie | 0.590** | 0.807*** | 0.178** | 0.236*** |
| | (0.250) | (0.294) | (0.073) | (0.077) |
| Panel B: using a time period of 6 months to distinguish between new/established coaches and recent/past ties | | | | |
| *Subgroups of tie-transfers:* | | | | |
| New coach/past tie | 0.093 | 0.137 | 0.035 | 0.045 |
| | (0.073) | (0.098) | (0.023) | (0.031) |
| New coach/recent tie | –0.239** | –0.444*** | –0.076* | –0.143*** |
| | (0.122) | (0.127) | (0.044) | (0.047) |
| Established coach/past tie | 0.114** | 0.201*** | 0.036** | 0.065*** |
| | (0.048) | (0.065) | (0.016) | (0.021) |
| Established coach/recent tie[a] | n.a. | n.a. | n.a. | n.a. |
| Control variables | Yes | Yes | Yes | Yes |
| Club fixed-effects | Yes | Yes | Yes | Yes |
| Coach fixed-effects | Yes | Yes | Yes | Yes |
| Season fixed-effects | Yes | Yes | Yes | Yes |
| Observations | 2,313 | 1,197 | 2,313 | 1,197 |

**Notes:** OLS estimates for Eq. (1) with a decomposed $\textit{tie-transfer}_{pcs}$ variable are displayed. In Panel A, a cutoff date of 12 months before the transfer is used to distinguish new/established coaches and recent/past ties. In Panel B, a cutoff date of 6 months before the transfer is used to distinguish new/established coaches and recent/past ties. All estimations also included a constant (not reported). Robust standard errors that have been adjusted for clustering at the individual player level are given in parentheses. *, **, and *** denote statistical significance at the 10%, 5%, and 1% levels, respectively.

[a] Tie-transfers involving an established coach and a recent tie do not occur if using a time period of 6 months to distinguish between new/established coaches and recent/past ties. This is because established coaches are with the current team for more than 6 months and recent ties indicate that a coach must have worked with a player within the last 6 months. As players are allowed to move only every 6 months (i.e., in the subsequent transfer window), it is not possible for a transfer to fulfill both requirements at the same time.

**Table A.2**
Full list of tie-transfers in the German Bundesliga from the seasons 1995/96 to 2013/14.

| | Transfer overview | | | | | | Tie-transfer details | | | |
|---|---|---|---|---|---|---|---|---|---|---|
| Nr. | Season | Club | Coach | Player | Type | Fee in Mio € | Tie-transfer subgroup | Coach entry | Tie lag | Club of previous coach–player tie |
| 1 | 1997/98 | 1. FC Kaiserslautern | Otto Rehhagel | Ciriaco Sforza | Buy | 3.30 | New coach/past tie | $s-1$ | $s-2$ | Bayern Muenchen |
| 2 | 1998/99 | 1. FC Kaiserslautern | Otto Rehhagel | Hany Ramzy | Buy | 1.15 | Established coach/past tie | $s-2$ | $s-4$ | Werder Bremen |
| 3 | 1999/00 | 1. FC Kaiserslautern | Otto Rehhagel | Mario Basler | Buy | 0.75 | Established coach/past tie | $s-3$ | $s-5$ | Werder Bremen |
| 4 | 2004/05 | 1. FC Kaiserslautern | Kurt Jara | Ingo Hertzsch | Free-agent | – | New coach/past tie | $s-1$ | $s-2$ | Hamburger SV |
| 5 | 1996/97 | 1. FC Koeln | Peter Neururer | Michael Kostner | Buy | n.a. | New coach/past tie | $s-1$ | $s-4$ | 1. FC Saarbruecken |
| 6 | 1997/98 | 1. FC Koeln | Lorenz-G. Koestner | Dennis Grassow | Buy | n.a. | New coach/recent tie | $s-0$ | $s-1$ | SpVgg Unterhaching |
| 7 | 2003/04 | 1. FC Koeln | Friedhelm Funkel | Mustafa Dogan | Free-agent | – | Established coach/past tie | $s-2$ | $s-8$ | KFC Uerdingen 05 |
| 8 | 2003/04 | 1. FC Koeln | Friedhelm Funkel | Marius Ebbers | Free-agent | – | Established coach/past tie | $s-2$ | $s-5$ | MSV Duisburg |
| 9 | 2005/06 | 1. FC Koeln | Hanspeter Latour | Ricardo Cabanas | Buy | 0.65 | New coach/recent tie | $s-0$ | $s-0$ | Grasshoppers (CH) |
| 10 | 2005/06 | 1. FC Koeln | Hanspeter Latour | Marco Streller | Loan | – | New coach/past tie | $s-0$ | $s-3$ | FC Thun (CH);FC Basel (CH) |
| 11 | 1998/99 | 1. FC Nuernberg | Willi Reimann | Matthias Mauksch | Buy | n.a. | New coach/past tie | $s-0$ | $s-2$ | VfL Wolfsburg |
| 12 | 1998/99 | 1. FC Nuernberg | Friedel Rausch | Rene Van Eck | Free-agent | – | New coach/past tie | $s-0$ | $s-7$ | FC Luzern (CH) |
| 13 | 2007/08 | 1. FC Nuernberg | Hans Meyer | Peer Kluge | Free-agent | – | Established coach/past tie | $s-2$ | $s-5$ | Borussia Moenchengl. |
| 14 | 2011/12 | 1. FC Nuernberg | Dieter Hecking | Hanno Balitsch | Free-agent | – | Established coach/past tie | $s-2$ | $s-2$ | Hannover 96 |
| 15 | 1997/98 | 1860 Muenchen | Werner Lorant | Jochen Kientz | Buy | n.a. | Established coach/past tie | $s-5$ | $s-4$ | 1860 Muenchen |
| 16 | 2008/09 | TSG Hoffenheim | Ralf Rangnick | Timo Hildebrand | Free-agent | – | Established coach/past tie | $s-2$ | $s-8$ | VfB Stuttgart |
| 17 | 2012/13 | TSG Hoffenheim | Markus Babbel | Matthieu Delpierre | Free-agent | – | New coach/past tie | $s-1$ | $s-3$ | VfB Stuttgart |
| 18 | 2004/05 | Arminia Bielefeld | Uwe Rapolder | Marijo Maric | Buy | 0.15 | New coach/past tie | $s-1$ | $s-7$ | SV Waldhof Mannheim |
| 19 | 2004/05 | Arminia Bielefeld | Uwe Rapolder | Ervin Skela | Free-agent | – | New coach/past tie | $s-1$ | $s-4$ | SV Waldhof Mannheim |
| 20 | 2005/06 | Arminia Bielefeld | Thomas von Heesen | Artur Wichniarek | Free-agent | – | New coach/past tie | $s-1$ | $s-2$ | Arminia Bielefeld |
| 21 | 2006/07 | Arminia Bielefeld | Thomas von Heesen | Joerg Boehme | Free-agent | – | Established coach/past tie | $s-2$ | $s-8$ | Arminia Bielefeld |
| 22 | 2007/08 | Arminia Bielefeld | Ernst Middendorp | Rowen Fernandez | Free-agent | – | New coach/recent tie | $s-1$ | $s-1$ | Kaizer Chiefs (ZA) |
| 23 | 2007/08 | Arminia Bielefeld | Ernst Middendorp | Siyabonga Nkosi | Buy | 0.35 | New coach/recent tie | $s-1$ | $s-1$ | Kaizer Chiefs (ZA) |
| 24 | 2008/09 | Arminia Bielefeld | Michael Frontzeck | Nico Herzig | Free-agent | – | New coach/past tie | $s-1$ | $s-2$ | Alemannia Aachen |
| 25 | 1999/00 | Bayer Leverkusen | Christoph Daum | Thomas Brdaric | Free-agent | – | Established coach/past tie | $s-3$ | $s-6$ | VfB Stuttgart |
| 26 | 2000/01 | Bayer Leverkusen | Christoph Daum | Andreas Neuendorf | Buy | 0.80 | Established coach/past tie | $s-4$ | $s-4$ | Bayer Leverkusen |
| 27 | 2001/02 | Bayer Leverkusen | Klaus Toppmoeller | Yildiray Bastuerk | Buy | 2.75 | New coach/past tie | $s-0$ | $s-3$ | VfL Bochum |
| 28 | 2004/05 | Bayer Leverkusen | Klaus Augenthaler | Jacek Krzynowek | Free-agent | – | Established coach/past tie | $s-2$ | $s-2$ | 1. FC Nuernberg |
| 29 | 2006/07 | Bayer Leverkusen | Michael Skibbe | Sergej Barbarez | Free-agent | – | New coach/past tie | $s-1$ | $s-7$ | Borussia Dortmund |
| 30 | 2011/12 | Bayer Leverkusen | Robin Dutt | Oemer Toprak | Buy | 3.00 | New coach/recent tie | $s-0$ | $s-1$ | SC Freiburg |
| 31 | 2013/14 | Bayer Leverkusen | Sami Hyypiae | Eren Derdiyok | Loan | – | New coach/past tie | $s-0$ | $s-2$ | Bayer Leverkusen |
| 32 | 1995/96 | Bayern Muenchen | Otto Rehhagel | Andreas Herzog | Buy | 2.54 | New coach/recent tie | $s-0$ | $s-1$ | Werder Bremen |
| 33 | 2000/01 | Bayern Muenchen | Ottmar Hitzfeld | Ciriaco Sforza | Buy | 2.30 | Established coach/past tie | $s-2$ | $s-10$ | Grasshoppers (CH) |
| 34 | 2004/05 | Bayern Muenchen | Felix Magath | Torsten Frings | Buy | 9.25 | New coach/past tie | $s-0$ | $s-6$ | Werder Bremen |
| 35 | 2007/08 | Bayern Muenchen | Ottmar Hitzfeld | Ze Roberto | Loan | – | New coach/past tie | $s-1$ | $s-4$ | Bayern Muenchen |
| 36 | 2013/14 | Bayern Muenchen | Pep Guardiola | Thiago Alcantara | Buy | 25.00 | New coach/past tie | $s-0$ | $s-2$ | FC Barcelona (ES) |
| 37 | 1998/99 | Borussia Dortmund | Michael Skibbe | Jens Lehmann | Buy | 4.00 | New coach/past tie | $s-0$ | $s-11$ | Schalke 04 |
| 38 | 2004/05 | Borussia Dortmund | Bert van Marwijk | Euzebiusz Smolarek | Loan | – | New coach/recent tie | $s-0$ | $s-1$ | Feyenoord Rotterdam (NL) |
| 39 | 2005/06 | Borussia Dortmund | Bert van Marwijk | Matthew Amoah | Buy | 0.40 | New coach/past tie | $s-1$ | $s-6$ | Fortuna Sittard (NL) |
| 40 | 2008/09 | Borussia Dortmund | Juergen Klopp | Neven Subotic | Buy | 4.50 | New coach/recent tie | $s-0$ | $s-1$ | FSV Mainz 05 |
| 41 | 2008/09 | Borussia Dortmund | Juergen Klopp | Mohamed Zidan | Buy | 2.80 | New coach/past tie | $s-0$ | $s-2$ | FSV Mainz 05 |
| 42 | 2009/10 | Borussia Dortmund | Juergen Klopp | Markus Feulner | Free-agent | – | New coach/past tie | $s-1$ | $s-2$ | FSV Mainz 05 |
| 43 | 2010/11 | Borussia Dortmund | Juergen Klopp | Antonio Da Silva | Free-agent | – | Established coach/past tie | $s-2$ | $s-5$ | FSV Mainz 05 |
| 44 | 2012/13 | Borussia Dortmund | Juergen Klopp | Nuri Sahin | Loan | – | Established coach/past tie | $s-4$ | $s-2$ | Borussia Dortmund |
| 45 | 2013/14 | Borussia Dortmund | Juergen Klopp | Manuel Friedrich | Free-agent | – | Established coach/past tie | $s-5$ | $s-7$ | FSV Mainz 05 |
| 46 | 2001/02 | Borussia Moenchengl. | Hans Meyer | Marco Kuentzel | Buy | 0.25 | Established coach/past tie | $s-2$ | $s-6$ | Union Berlin |
| 47 | 2002/03 | Borussia Moenchengl. | Hans Meyer | Marcel Ketelaer | Loan | – | Established coach/past tie | $s-3$ | $s-3$ | Borussia Moenchengl. |
| 48 | 2003/04 | Borussia Moenchengl. | Ewald Lienen | Pascal Ojigwe | Free-agent | – | New coach/past tie | $s-1$ | $s-4$ | 1. FC Koeln |
| 49 | 2004/05 | Borussia Moenchengl. | Dick Advocaat | Craig Moore | Free-agent | – | New coach/past tie | $s-0$ | $s-3$ | Glasgow Rangers (GB) |
| 50 | 2006/07 | Borussia Moenchengl. | Jupp Heynckes | Michael Delura | Loan | – | New coach/past tie | $s-0$ | $s-2$ | Schalke 04 |
| 51 | 2006/07 | Borussia Moenchengl. | Jupp Heynckes | Christofer Heimeroth | Buy | 0.75 | New coach/past tie | $s-0$ | $s-2$ | Schalke 04 |
| 52 | 2008/09 | Borussia Moenchengl. | Hans Meyer | Tomas Galasek | Buy | 0.05 | New coach/recent tie | $s-0$ | $s-1$ | 1. FC Nuernberg |
| 53 | 2009/10 | Borussia Moenchengl. | Michael Frontzeck | Thorben Marx | Free-agent | – | New coach/recent tie | $s-0$ | $s-1$ | Arminia Bielefeld |
| 54 | 2013/14 | Borussia Moenchengl. | Lucien Favre | Raffael | Buy | 5.00 | Established coach/past tie | $s-3$ | $s-4$ | Hertha BSC;FC Zuerich (CH) |
| 55 | 2013/14 | Eintracht Braunschweig | Torsten Lieberknecht | Karim Bellarabi | Loan | – | Established coach/past tie | $s-6$ | $s-3$ | Eintracht Braunschweig |
| 56 | 2013/14 | Eintracht Braunschweig | Torsten Lieberknecht | Torsten Oehrl | Buy | 0.50 | Established coach/past tie | $s-6$ | $s-6$ | Eintracht Braunschweig |

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Table A.2 – *Continued from previous page*

| | Transfer overview | | | | | | Tie-transfer details | | | |
|---|---|---|---|---|---|---|---|---|---|---|
| Nr. | Season | Club | Coach | Player | Type | Fee in Mio € | Tie-transfer subgroup | Coach entry | Tie lag | Club of previous coach–player tie |
| 57 | 1999/00 | Eintracht Frankfurt | Joerg Berger | Rolf-C. Guie-Mien | Buy | 2.50 | New coach/recent tie | $s-1$ | $s-1$ | Karlsruher SC |
| 58 | 1999/00 | Eintracht Frankfurt | Joerg Berger | Horst Heldt | Buy | 1.80 | New coach/past tie | $s-1$ | $s-7$ | 1. FC Koeln |
| 59 | 2000/01 | Eintracht Frankfurt | Felix Magath | Sasa Ciric | Free-agent | – | New coach/past tie | $s-1$ | $s-3$ | 1. FC Nuernberg |
| 60 | 2009/10 | Eintracht Frankfurt | Michael Skibbe | Pirmin Schwegler | Buy | 0.50 | New coach/past tie | $s-0$ | $s-2$ | Bayer Leverkusen |
| 61 | 2010/11 | Eintracht Frankfurt | Michael Skibbe | Theofanis Gekas | Buy | 1.00 | New coach/past tie | $s-1$ | $s-3$ | Bayer Leverkusen |
| 62 | 2012/13 | Eintracht Frankfurt | Armin Veh | Martin Lanig | Free-agent | – | New coach/past tie | $s-1$ | $s-4$ | VfB Stuttgart |
| 63 | 2013/14 | Eintracht Frankfurt | Armin Veh | Alexander Madlung | Free-agent | – | Established coach/past tie | $s-2$ | $s-4$ | VfL Wolfsburg |
| 64 | 2013/14 | Eintracht Frankfurt | Armin Veh | Tobias Weis | Loan | – | Established coach/past tie | $s-2$ | $s-7$ | VfB Stuttgart |
| 65 | 2012/13 | FC Augsburg | Markus Weinzierl | Ronny Philp | Free-agent | – | New coach/recent tie | $s-0$ | $s-1$ | Jahn Regensburg |
| 66 | 2013/14 | FC Augsburg | Markus Weinzierl | Dong-Won Ji | Buy | n.a. | New coach/recent tie | $s-1$ | $s-1$ | FC Augsburg |
| 67 | 2004/05 | FSV Mainz 05 | Juergen Klopp | Michael Thurk | Buy | 0.20 | Established coach/recent tie | $s-4$ | $s-1$ | FSV Mainz 05 |
| 68 | 2006/07 | FSV Mainz 05 | Juergen Klopp | Mimoun Azaouagh | Loan | – | Established coach/past tie | $s-6$ | $s-2$ | FSV Mainz 05 |
| 69 | 2006/07 | FSV Mainz 05 | Juergen Klopp | Mohamed Zidan | Buy | 2.08 | Established coach/recent tie | $s-6$ | $s-1$ | FSV Mainz 05 |
| 70 | 1995/96 | Fortuna Duesseldorf | Aleksandar Ristic | Thomas Seeliger | Buy | 0.15 | Established coach/past tie | $s-3$ | $s-7$ | Fortuna Duesseldorf |
| 71 | 2012/13 | Fortuna Duesseldorf | Norbert Meier | Andrej Voronin | Loan | – | Established coach/past tie | $s-5$ | $s-15$ | Borussia Moenchengl. |
| 72 | 1998/99 | Hamburger SV | Frank Pagelsdorf | Martin Groth | Free-agent | – | New coach/past tie | $s-1$ | $s-2$ | Hansa Rostock |
| 73 | 2000/01 | Hamburger SV | Frank Pagelsdorf | Sergej Barbarez | Buy | 1.80 | Established coach/past tie | $s-3$ | $s-4$ | Hansa Rostock;Union Berlin |
| 74 | 2001/02 | Hamburger SV | Frank Pagelsdorf | Martin Pieckenhagen | Free-agent | – | Established coach/past tie | $s-4$ | $s-5$ | Hansa Rostock;Union Berlin |
| 75 | 2002/03 | Hamburger SV | Kurt Jara | Michael Baur | Free-agent | – | New coach/recent tie | $s-1$ | $s-1$ | Wacker Innsbruck (AT) |
| 76 | 2003/04 | Hamburger SV | Klaus Toppmoeller | Tom Starke | Loan | – | New coach/recent tie | $s-0$ | $s-1$ | Bayer Leverkusen |
| 77 | 2013/14 | Hamburger SV | Thorsten Fink | Jacques Zoua | Buy | 0.60 | Established coach/past tie | $s-2$ | $s-2$ | FC Basel (CH) |
| 78 | 2002/03 | Hannover 96 | Ralf Rangnick | Fredi Bobic | Buy | 0.50 | New coach/past tie | $s-1$ | $s-4$ | VfB Stuttgart |
| 79 | 2002/03 | Hannover 96 | Ralf Rangnick | Kai Oswald | Free-agent | – | New coach/past tie | $s-1$ | $s-4$ | VfB Stuttgart |
| 80 | 2003/04 | Hannover 96 | Ralf Rangnick | Thomas Schneider | Free-agent | – | Established coach/past tie | $s-2$ | $s-3$ | VfB Stuttgart |
| 81 | 2003/04 | Hannover 96 | Ralf Rangnick | Jan Simak | Loan | – | Established coach/recent tie | $s-2$ | $s-1$ | Hannover 96 |
| 82 | 2003/04 | Hannover 96 | Ralf Rangnick | Marc Ziegler | Loan | – | Established coach/past tie | $s-2$ | $s-5$ | VfB Stuttgart |
| 83 | 2004/05 | Hannover 96 | Ewald Lienen | Veljko Paunovic | Loan | – | New coach/past tie | $s-1$ | $s-2$ | CD Teneriffa (ES) |
| 84 | 2004/05 | Hannover 96 | Ewald Lienen | Michael Tarnat | Free-agent | – | New coach/past tie | $s-1$ | $s-11$ | MSV Duisburg |
| 85 | 2005/06 | Hannover 96 | Ewald Lienen | Hanno Balitsch | Buy | 1.00 | Established coach/past tie | $s-2$ | $s-4$ | 1. FC Koeln |
| 86 | 2005/06 | Hannover 96 | Ewald Lienen | Thomas Brdaric | Buy | 1.80 | Established coach/past tie | $s-2$ | $s-2$ | Hannover 96 |
| 87 | 2006/07 | Hannover 96 | Peter Neururer | Frank Fahrenhorst | Free-agent | – | New coach/past tie | $s-1$ | $s-3$ | VfL Bochum |
| 88 | 2007/08 | Hannover 96 | Dieter Hecking | Sergio Pinto | Free-agent | – | New coach/recent tie | $s-1$ | $s-1$ | Alemannia Aachen |
| 89 | 2008/09 | Hannover 96 | Dieter Hecking | Jan Schlaudraff | Buy | 2.00 | Established coach/past tie | $s-2$ | $s-2$ | Alemannia Aachen |
| 90 | 2011/12 | Hannover 96 | Mirko Slomka | Christian Pander | Free-agent | – | Established coach/past tie | $s-2$ | $s-4$ | Schalke 04 |
| 91 | 1995/96 | Hansa Rostock | Frank Pagelsdorf | Christian Beeck | Buy | 0.08 | New coach/past tie | $s-1$ | $s-2$ | Union Berlin |
| 92 | 1995/96 | Hansa Rostock | Frank Pagelsdorf | Goran Markov | Buy | 0.08 | New coach/past tie | $s-1$ | $s-2$ | Union Berlin |
| 93 | 1995/96 | Hansa Rostock | Frank Pagelsdorf | Dirk Rehbein | Buy | 0.08 | New coach/past tie | $s-1$ | $s-2$ | Union Berlin |
| 94 | 1996/97 | Hansa Rostock | Frank Pagelsdorf | Sergej Barbarez | Free-agent | – | Established coach/past tie | $s-2$ | $s-3$ | Union Berlin |
| 95 | 1996/97 | Hansa Rostock | Frank Pagelsdorf | Martin Pieckenhagen | Buy | 0.08 | Established coach/past tie | $s-2$ | $s-4$ | Union Berlin |
| 96 | 1996/97 | Hansa Rostock | Frank Pagelsdorf | Marko Rehmer | Buy | 0.20 | Established coach/past tie | $s-2$ | $s-3$ | Union Berlin |
| 97 | 2000/01 | Hansa Rostock | Friedhelm Funkel | Bachirou Salou | Loan | – | New coach/past tie | $s-0$ | $s-3$ | MSV Duisburg |
| 98 | 2001/02 | Hansa Rostock | Friedhelm Funkel | Markus Beierle | Buy | 0.50 | New coach/past tie | $s-1$ | $s-2$ | MSV Duisburg |
| 99 | 2001/02 | Hansa Rostock | Friedhelm Funkel | Dietmar Hirsch | Buy | 0.38 | New coach/past tie | $s-1$ | $s-2$ | MSV Duisburg |
| 100 | 2002/03 | Hansa Rostock | Armin Veh | Godfried Aduobe | Free-agent | – | New coach/recent tie | $s-1$ | $s-1$ | SSV Reutlingen |
| 101 | 2002/03 | Hansa Rostock | Armin Veh | Michal Kovar | Buy | 0.05 | New coach/recent tie | $s-1$ | $s-1$ | SSV Reutlingen |
| 102 | 2007/08 | Hansa Rostock | Frank Pagelsdorf | Gledson | Buy | 0.20 | Established coach/recent tie | $s-2$ | $s-1$ | Hansa Rostock |
| 103 | 2007/08 | Hansa Rostock | Frank Pagelsdorf | Addy-Waku Menga | Buy | 0.25 | Established coach/past tie | $s-2$ | $s-4$ | VfL Osnabrueck |
| 104 | 2007/08 | Hansa Rostock | Frank Pagelsdorf | Stefan Waechter | Free-agent | – | Established coach/past tie | $s-2$ | $s-6$ | Hamburger SV |
| 105 | 2001/02 | Hertha BSC | Juergen Roeber | Andreas Neuendorf | Free-agent | – | Established coach/past tie | $s-6$ | $s-2$ | Hertha BSC |
| 106 | 2007/08 | Hertha BSC | Lucien Favre | Raffael | Buy | 4.30 | New coach/recent tie | $s-0$ | $s-1$ | FC Zuerich (CH) |
| 107 | 2007/08 | Hertha BSC | Lucien Favre | Steve Von Bergen | Buy | 1.50 | New coach/recent tie | $s-0$ | $s-1$ | FC Zuerich (CH) |
| 108 | 2009/10 | Hertha BSC | Lucien Favre | Cesar | Free-agent | – | Established coach/past tie | $s-2$ | $s-3$ | FC Zuerich (CH) |
| 109 | 2013/14 | Hertha BSC | Jos Luhukay | Alexander Baumjohann | Free-agent | – | New coach/past tie | $s-1$ | $s-5$ | Borussia Moenchengl. |
| 110 | 2013/14 | Hertha BSC | Jos Luhukay | Hajime Hosogai | Buy | 1.00 | New coach/past tie | $s-1$ | $s-2$ | FC Augsburg |
| 111 | 2013/14 | Hertha BSC | Jos Luhukay | Sebastian Langkamp | Free-agent | – | New coach/past tie | $s-1$ | $s-2$ | FC Augsburg |
| 112 | 2013/14 | Hertha BSC | Jos Luhukay | Johannes V. den Bergh | Free-agent | – | New coach/past tie | $s-1$ | $s-5$ | Borussia Moenchengl. |
| 113 | 2008/09 | Karlsruher SC | Edmund Becker | Giovanni Federico | Loan | – | Established coach/past tie | $s-4$ | $s-2$ | Karlsruher SC |

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Table A.2 – *Continued from previous page*

| | Transfer overview | | | | | | Tie-transfer details | | | |
|---|---|---|---|---|---|---|---|---|---|---|
| Nr. | Season | Club | Coach | Player | Type | Fee in Mio € | Tie-transfer subgroup | Coach entry | Tie lag | Club of previous coach–player tie |
| 114 | 1996/97 | MSV Duisburg | Friedhelm Funkel | Horst Steffen | Buy | 0.30 | New coach/recent tie | $s-1$ | $s-1$ | KFC Uerdingen 05 |
| 115 | 1998/99 | MSV Duisburg | Friedhelm Funkel | Marcus Wedau | Buy | 0.50 | Established coach/past tie | $s-3$ | $s-3$ | KFC Uerdingen 05 |
| 116 | 1999/00 | MSV Duisburg | Friedhelm Funkel | Michael Zeyer | Free-agent | – | Established coach/past tie | $s-4$ | $s-2$ | MSV Duisburg |
| 117 | 2005/06 | MSV Duisburg | Norbert Meier | Markus Hausweiler | Free-agent | – | Established coach/past tie | $s-3$ | $s-8$ | Borussia Moenchengl. |
| 118 | 1999/00 | SC Freiburg | Volker Finke | Andreas Zeyer | Buy | n.a. | Established coach/past tie | $s-8$ | $s-3$ | SC Freiburg |
| 119 | 2004/05 | SC Freiburg | Volker Finke | Regis Dorn | Free-agent | – | Established coach/past tie | $s-13$ | $s-3$ | SC Freiburg |
| 120 | 2009/10 | SC Freiburg | Robin Dutt | Manuel Salz | Free-agent | – | Established coach/past tie | $s-2$ | $s-3$ | Stuttgarter Kickers |
| 121 | 1999/00 | SSV Ulm 1846 | Martin Andermatt | Rui Marques | Free-agent | – | New coach/recent tie | $s-1$ | $s-1$ | FC Baden (CH) |
| 122 | 1996/97 | Schalke 04 | Huub Stevens | Marco V. Hoogdalem | Buy | n.a. | New coach/recent tie | $s-0$ | $s-0$ | Roda JC Kerkrade (NL) |
| 123 | 2000/01 | Schalke 04 | Huub Stevens | Mike Bueskens | Free-agent | – | Established coach/recent tie | $s-4$ | $s-1$ | Schalke 04 |
| 124 | 2001/02 | Schalke 04 | Huub Stevens | Marc Wilmots | Buy | 2.10 | Established coach/past tie | $s-5$ | $s-2$ | Schalke 04 |
| 125 | 2008/09 | Schalke 04 | Fred Rutten | Orlando Engelaar | Buy | 5.50 | New coach/recent tie | $s-0$ | $s-1$ | Twente Enschede (NL) |
| 126 | 2011/12 | Schalke 04 | Ralf Rangnick | Timo Hildebrand | Free-agent | – | New coach/past tie | $s-1$ | $s-2$ | TSG Hoffenheim;VfB Stuttgart |
| 127 | 2012/13 | Schalke 04 | Huub Stevens | Ibrahim Afellay | Loan | – | New coach/past tie | $s-1$ | $s-4$ | PSV Eindhoven (NL) |
| 128 | 1999/00 | SpVgg Unterhaching | Lorenz-G. Koestner | Dennis Grassow | Buy | 0.30 | New coach/past tie | $s-1$ | $s-2$ | 1. FC Koeln;SpVgg Unterhaching |
| 129 | 2000/01 | SpVgg Unterhaching | Lorenz-G. Koestner | Abdelaziz Ahanfouf | Buy | 0.13 | Established coach/past tie | $s-2$ | $s-2$ | SpVgg Unterhaching |
| 130 | 2002/03 | VfB Stuttgart | Felix Magath | Horst Heldt | Free-agent | – | Established coach/past tie | $s-2$ | $s-2$ | Eintracht Frankfurt |
| 131 | 2002/03 | VfB Stuttgart | Felix Magath | Michael Mutzel | Free-agent | – | Established coach/past tie | $s-2$ | $s-2$ | Eintracht Frankfurt |
| 132 | 2010/11 | VfB Stuttgart | Christian Gross | Philipp Degen | Loan | – | New coach/past tie | $s-1$ | $s-6$ | FC Basel (CH) |
| 133 | 2012/13 | VfB Stuttgart | Bruno Labbadia | Tunay Torun | Free-agent | – | Established coach/past tie | $s-2$ | $s-3$ | Hamburger SV |
| 134 | 1997/98 | VfL Bochum | Klaus Toppmoeller | Norbert Hofmann | Free-agent | – | Established coach/past tie | $s-3$ | $s-5$ | SV Waldhof Mannheim |
| 135 | 1997/98 | VfL Bochum | Klaus Toppmoeller | Mirko Reichel | Buy | n.a. | Established coach/past tie | $s-3$ | $s-7$ | FC Erzgebirge Aue |
| 136 | 1998/99 | VfL Bochum | Klaus Toppmoeller | Mirko Dickhaut | Free-agent | – | Established coach/past tie | $s-4$ | $s-5$ | Eintracht Frankfurt |
| 137 | 1998/99 | VfL Bochum | Klaus Toppmoeller | Maurizio Gaudino | Buy | 0.13 | Established coach/past tie | $s-4$ | $s-5$ | Eintracht Frankfurt |
| 138 | 1998/99 | VfL Bochum | Klaus Toppmoeller | Andreas Zeyer | Buy | n.a. | Established coach/past tie | $s-4$ | $s-10$ | SSV Ulm 1846 |
| 139 | 2002/03 | VfL Bochum | Peter Neururer | Sunday Oliseh | Loan | – | New coach/past tie | $s-1$ | $s-6$ | 1. FC Koeln |
| 140 | 2003/04 | VfL Bochum | Peter Neururer | Thomas Zdebel | Free-agent | – | Established coach/past tie | $s-2$ | $s-6$ | 1. FC Koeln |
| 141 | 2006/07 | VfL Bochum | Marcel Koller | Alexander Bade | Free-agent | – | New coach/past tie | $s-1$ | $s-3$ | 1. FC Koeln |
| 142 | 2006/07 | VfL Bochum | Marcel Koller | Oliver Schroeder | Free-agent | – | New coach/past tie | $s-1$ | $s-3$ | 1. FC Koeln |
| 143 | 1999/00 | VfL Wolfsburg | Wolfgang Wolf | Jonathan Akpoborie | Buy | 0.95 | Established coach/past tie | $s-2$ | $s-5$ | Stuttgarter Kickers |
| 144 | 1999/00 | VfL Wolfsburg | Wolfgang Wolf | Zoltan Sebescen | Buy | 0.43 | Established coach/past tie | $s-2$ | $s-2$ | Stuttgarter Kickers |
| 145 | 2000/01 | VfL Wolfsburg | Wolfgang Wolf | Tomislav Maric | Free-agent | – | Established coach/past tie | $s-3$ | $s-3$ | Stuttgarter Kickers |
| 146 | 2002/03 | VfL Wolfsburg | Wolfgang Wolf | Roy Praeger | Free-agent | – | Established coach/past tie | $s-5$ | $s-4$ | VfL Wolfsburg |
| 147 | 2004/05 | VfL Wolfsburg | Eric Gerets | Kevin Hofland | Buy | 2.00 | New coach/past tie | $s-1$ | $s-3$ | PSV Eindhoven (NL) |
| 148 | 2004/05 | VfL Wolfsburg | Eric Gerets | Marian Hristov | Buy | 1.00 | New coach/recent tie | $s-1$ | $s-1$ | 1. FC Kaiserslautern |
| 149 | 2006/07 | VfL Wolfsburg | Klaus Augenthaler | Jacek Krzynowek | Buy | 1.35 | New coach/recent tie | $s-1$ | $s-1$ | Bayer Leverkusen;1. FC Nuernb. |
| 150 | 2007/08 | VfL Wolfsburg | Felix Magath | Diego Benaglio | Buy | 1.50 | New coach/past tie | $s-0$ | $s-4$ | VfB Stuttgart |
| 151 | 2011/12 | VfL Wolfsburg | Felix Magath | Aleksandr Hleb | Loan | – | New coach/past tie | $s-1$ | $s-8$ | VfB Stuttgart |
| 152 | 2011/12 | VfL Wolfsburg | Felix Magath | Hasan Salihamidzic | Free-agent | – | New coach/past tie | $s-1$ | $s-5$ | Bayern Muenchen;Hamburger SV |
| 153 | 2013/14 | VfL Wolfsburg | Dieter Hecking | Timm Klose | Buy | 6.00 | New coach/recent tie | $s-1$ | $s-1$ | 1. FC Nuernberg |
| 154 | 2005/06 | Werder Bremen | Thomas Schaaf | Torsten Frings | Buy | 5.00 | Established coach/past tie | $s-7$ | $s-4$ | Werder Bremen |
| 155 | 2008/09 | Werder Bremen | Thomas Schaaf | Claudio Pizarro | Loan | – | Established coach/past tie | $s-10$ | $s-8$ | Werder Bremen |
| 156 | 2009/10 | Werder Bremen | Thomas Schaaf | Tim Borowski | Buy | 0.75 | Established coach/past tie | $s-11$ | $s-2$ | Werder Bremen |
| 157 | 2013/14 | Werder Bremen | Robin Dutt | Cedrick Makiadi | Buy | 3.00 | New coach/past tie | $s-0$ | $s-3$ | SC Freiburg |

**Notes:** The list includes all transfers in the German Bundesliga from the seasons 1995/96 to 2013/14 in which the involved coach and player have previously worked together in a coach–player relationship during their professional careers. To be included in the list, the player involved in a transfer must have played for at least 30 minutes in a single match during the season of the transfer. Not included are tie-transfers in which players move back to the former club after an ended loan arrangement.

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# A.3
# Does sports activity improve health? Representative evidence using proximity to sports facilities as an instrument*

**Abstract**

Using representative and geocoded data from the Swiss Household Panel and the Swiss Business Census, we estimate the effect of sports activity on health care utilization and health. Because sports activity is likely correlated with unobserved determinants of health care utilization and health, we use the number of sports facilities within 6 miles of the individual's residence as an instrument. We find that doing sports at least once a week significantly reduces the number of doctor visits, overweight and sleeping problems. The magnitudes of these effects are larger in the IV estimations than in OLS estimations, which are biased toward zero due to reporting errors in sports activity and an omitted variable bias. To know the magnitudes of the causal effects is crucial for any kind of cost-benefit analysis of promoting individual sports activity.

**JEL Classification**: I10; I12; H51; C26
**Keywords**: sports activity; health care utilization; health; instrumental variable; proximity to sports facilities

*This paper has been written jointly with Stephan Nüesch and Egon Franck.

# 1 Introduction

Physical inactivity is widely acknowledged as a global health problem in the 21st century. The proportion of inactive people is rising in many countries, creating risks for individual health, health care utilization and ultimately public health care costs (World Health Organization, 2010). Therefore, exercise and intervention programs that target an increase of individual physical activity are a recurring theme on the agenda of policy makers around the world (Heath et al., 2012). Such programs are supported by a rich body of cross-sectional epidemiological research showing a positive correlation between physical inactivity and a wide variety of detrimental health outcomes such as obesity, hypertension, osteoporosis, osteoarthritis, diabetes mellitus, colon and breast cancer, depression (see e.g., Warburton, Nicol, & Bredin, 2006; Katzmarzyk & Janssen, 2004) and health care utilization such as doctor consultations and hospital days (see e.g., Manning, Keeler, Newhouse, Sloss, & Wasserman, 1991; Haapanen-Niemi, Miilunpalo, Vuori, Pasanen, & Oja, 1999; Katzmarzyk, Gledhill, & Shephard, 2000; Sari, 2009).

However, because physical activity is an endogenous choice variable and therefore likely correlated with unobservable confounders, evidence from correlational studies cannot be given a causal interpretation. For example, health-conscious people with a high level of body awareness may be more active. At the same time, such people also tend to get more health screenings (e.g., cancer screening or general health checks) and tend to visit the doctor more often (Ioannou, Chapko, & Dominitz, 2003; Hansell, Sherman, & Mechanic, 1991). Another potential confounder is a person's healthy or unhealthy lifestyle, for example her nutrition, sleeping behaviour or personal hygiene.

A healthy lifestyle tends to be positively correlated with sports activity and negatively correlated with health issues and health care utilization.

Randomized control trials can potentially solve the endogeneity issue by assigning individuals to treatment groups with an intervention program or to control groups. Field experiments on physical activity and health-related outcomes have been conducted with Texaco employees (Baun, Bemacki, & Tsai, 1986), employees of insurance companies (Shephard, 1992), Bank of America retirees (Leigh et al., 1992), or Johnson and Johnson employees (Ozminkowski et al., 2002). But because samples in these studies are small and derived from very specific settings, results from these experimental studies are hardly generalizable to the rest of the population (Sari, 2009).

In this study, we take advantage of two hitherto uncombined data sets from Switzerland to address both the endogeneity and the external reliability issues. We combine representative survey data on individual sports activity and health-related outcomes with data on sports infrastructure. Employing geographic coordinates of individual home addresses and units of sports facilities, we use the availability of sports facilities to predict sports activity. Geographic proximity to sports facilities is an ideal instrument because it increases sports activity, and the supply of sports facilities is exogenous to unobservable factors affecting health and health care utilization (at the individual level).

Our identification strategy is related to the work of Huang and Humphreys (2012), who use proximity to sports facilities to identify the effect of sports activity on happiness, and Bowblis and McHone (2013) and Grabowski, Feng, Hirth, Rahman, and Mor (2013), who use proximity to nursing homes with different ownership to test the influence of nursing ownership on care quality. We are the first to use geographic proximity to sports facilities as an instrument in the context of sports activity and health.

We find that doing sports at least once a week reduces the number of doctor visits and the number of hospital days. The magnitudes of these effects are larger in our estimations using instrumental variable (IV) models than in those using non-IV models and in related correlational studies (Sari, 2009; Haapanen-Niemi et al., 1999; Keeler, Manning, Newhouse, Sloss, & Wasserman, 1989). When we use proximity to sports facilities as an instrument for sports activity, individuals who do sports at least once a week have 23 percent of the doctor visits and 43 percent of the number of hospital days of inactive individuals (although the latter effect is not statistically significant due to the high standard errors in the IV model).

Because self-reported sports activity information likely suffers from misreporting (Ferrari, Friedenreich, & Matthews, 2007), we argue that non-IV estimates on the effect of sports activity on health care utilization are biased towards zero. IV models provide a solution to the errors-in-variables problem and the resulting attenuation bias. In addition, the non-IV models may also underestimate the effects of sports activity on health care utilization due to a positive omitted variable bias. For example, individuals who do sports at least once a week may be more health-conscious than non-active individuals, and (unobserved) health-consciousness increases health care utilization, holding everything else equal (Ioannou et al., 2003; Hansell et al., 1991).

In order to examine the channels through which sports activity influences health care utilization, we estimate how sports activity affects four specific health outcomes: overweight, sleeping problems, headaches, and back problems. Our IV results confirm findings from previous correlational studies showing that sports activity significantly reduces overweight (Janssen et al., 2005; Ortega, Ruiz, & Sjöström, 2007; Patrick et al., 2004) and sleeping problems (Atkinson & Davenne, 2007).

While the effects of sports activity on headaches and back problems are also negative and significant in the non-IV models, they become statistically insignificant when instrumenting sports activity by proximity to sports facilities. This indicates a reverse causation issue in the non-IV models. Using the proximity to sports facilities as an instrument of sports activity addresses the reverse causation issue as headaches and back problems decrease the propensity to do sports. Notably, the insignificant effect of sports activity on back problems is in line with comprehensive evidence from a recent medical review study (Sitthipornvorakul, Janwantanakul, Purepong, Pensri, & van der Beek, 2011).

The remainder of this paper is structured as follows: In Section 2, we outline our data and the empirical strategy. In Section 3, we explain our estimation method. In Section 4, we present the results, and Section 5 concludes.

## 2 Data and empirical strategy

The empirical problems of disentangling the relationship between individual sports activity and health-related outcomes are manifest. On the one hand, if one relies on observable field data from representative samples, self-selection is a major issue because sports activity is an endogenous choice variable. Failure to account for this source of endogeneity will bias any estimation of an effect from individual sports activity (see e.g., Heckman, 1979). On the other hand, if one relies on quasi-experimental clinical trials that allow for randomization of fitness program participants and control groups, findings are hardly representative for general populations (Sari, 2009).

To consider both issues at once, we use representative field data on individual sports activity and health-related outcomes and address the self-selection problem by employing an instrumental variables strategy. We use variation in geographic proximity to sports facilities as an instrument for individual sports activity. The reasoning behind this strategy is that living close to sports facilities implies easier access to sports infrastructure (Huang & Humphreys, 2012) and reduces the "costs" of doing sports. Both monetary costs (in terms of transportation costs) and time costs (for travelling) indicate a positive relation between short distances to sports facilities and sports activity (see also the discussion in Felfe, Lechner, and Steinmayr (2011))

In this section, we first describe our data sources. Second, we discuss the dependent and independent variables that we investigate in our analysis and third, we present and discuss our instrumental variable.

## 2.1 Description of data sources

The data on sports activity, health and health care utilization is part of the tenth wave of the *Swiss Household Panel* (SHP) collected in 2008. A key advantage of SHP is that the sample includes a stratified random sample of households representing the resident population of Switzerland. Originally, the randomization of the sample was constructed under guidance of the Swiss *Federal Statistical Office* based on the major statistical regions in Switzerland (for detailed information about the sample design, see Voorpostel et al., 2012). Overall, our sample comprises 6,872 individuals (aged 14 years and older) living in 4,166 distinct households. The data for these 6,872 individuals were collected using computer-assisted telephone interviews held from September

2008 to February 2009. The survey includes questions on individual sports activity, health and health care utilization, and other socioeconomic characteristics.[1]

To measure the availability of sport facilities for the individuals in the SHP sample, we obtained additional data from the Swiss *Business Census* for the year 2008. The *Business Census* is a mandatory survey of workplaces and businesses in Switzerland and aims to collect full data on their economic activity, the number of persons employed, and their exact geographic location.[2] The data is collected by means of paper questionnaires and online questionnaires under the responsibility of the Swiss *Federal Statistical Office*. The reference day for the 2008 *Business Census* was September 30, 2008.

A specific classification code of economic activity (called *NOGA* codes in the Swiss context) marks sport facilities. Under *NOGA* code 931100, facilities for indoor or outdoor sports are recorded. This includes football grounds, athletics grounds, swimming pools, golf courses and so on. In total, about one thousand sport facilities are recorded by *NOGA* code 931100. An important advantage is the geo-coding of each sports facility via Swiss grid coordinates. These coordinates pinpoint the location of a sports facility within a few meters of the building's midpoint and allow us to draw a very precise map of the geographic distribution of sports facilities in Switzerland.

In the standard version of SHP, the most accurate geographic information on an individual's home location is the canton of residence. However, to obtain an accurate link between SHP individuals and sports facilities, we needed more detailed geographic information. We gratefully acknowledge SHP's provision of exact home addresses for

[1] After dropping a small number of individuals that did not respond correctly to all of the items of our analysis, the final sample consists of 6,558 out of the original 6,872 individuals included in the SHP survey.

[2] Participation in the survey is compulsory for all targeted workplaces and businesses. However, there is a minimum of 20 hours of weekly work for a business unit to be targeted by the survey. Therefore, the data does not include very small sports facilities that do not employ at least one person with an engagement of 50 percent or more.

each individual in the data set, after we signed a special confidentiality agreement. The provided home addresses included information on the community, zip code, street name and street number.[3] We used the public webpage *http://tools.retorte.ch/map/* to transform this address data into Swiss grid coordinates. Using the home address Swiss grid coordinates, we are able to pinpoint linear distances between the residence of an individual and all sports facilities obtained from the Swiss *Business Census* with a precision of a few meters.

## 2.2 Health and health care utilization measures

The SHP survey includes two items on health care utilization. In a question on doctor visits, respondents were asked: "In the last 12 months, how many times have you consulted a doctor?" Doctor visits at home are explicitly included in these numbers (through the interviewers' introduction of the question), whereas visits to a dentist do not count. In a similar question on hospital services, respondents were asked: "In the last 12 months, how many days have you spent in a hospital or specialized clinic, not including spas or wellness cures?" Outcomes for both items are non-negative, integer count variables.

To examine the potential channels through which sports activity affects health care utilization, we also aim to test the effect of sports activity on various health outcomes. Following the questions included in the SHP survey, we consider four specific indicators for health problems. Most notably, we include a discrete indicator for overweight, which has been argued to be both a consequence of physical inactivity due to a disrupted energy balance (Katzmarzyk & Janssen, 2004) *and* a risk factor for chronic

[3]SHP was not able to provide the complete address for 43 individuals (either no street name was provided or the provided street name was not identifiable). In these cases, we were not able to obtain Swiss grid coordinates. Hence, we were not able to match these individuals with the sports infrastructure data and were forced to exclude them from our sample.

health problems (Dixon, 2010) and health care utilization (Cawley & Meyerhoefer, 2012). To identify overweight individuals, we converted height and weight data into a discrete measure of overweight via WHO Body Mass Index guidelines (World Health Organization, 2000).

Other specific indicators available from the SHP survey include regular suffering from sleeping problems, headaches, and back problems. They were obtained from questions of the type: "During the last 4 weeks, have you suffered from one of the following disorders or health problems?" While respondents were allowed to choose between three categories (not at all, somewhat, very much), we used a binary yes/no coding that only treats serious incidences (i.e., very much) as a specific health problem.

## 2.3 Sports activity measure

To identify individual sports activity, we draw on an SHP question from the leisure time section. Respondents were asked: "How frequently do you practice an individual or team sport (for example fitness, jogging, football, volley ball, tennis)?" Respondents were free to provide any description of their sports activity level but interviewers were supposed to help respondents provide a reasonable answer if necessary. Afterwards, interviewers had to assign the responses to five different levels of sports activity: every day, at least once a week, at least once a month, less then once a month, never. Large proportions of the respondents reported doing sports activities at least once a week (57.9%) or not at all (25.6%). Each of the other three categories contained only a small proportion of the respondents: 6.9 percent reported daily sports activity, and 9.5 percent reported some occasional sports activity but not every week (at least once a month: 7.1%; less than once a month: 2.4%). To allow for a straighforward interpretation of the results, we aggregate the five categories of sports activity into the discrete measure of sports activity "at least once a week".[4]

## 2.4 Instrumental variable: proximity to sports facilities

To mimic randomization of individuals' selection into sports activity, we use geographic proximity to sports facilities as an instrument. We define *proximity* to sports facilities as the number of sports facilities within a certain radius surrounding an individual's home address. The key issue in the construction of the measure is to identify an appropriate radius up to which sports facilities potentially affect a person's sports activity.

Table 1 shows summary statistics for different distance boundaries (1 mile to 10 miles) and the $F$-statistics for the measures in first-stage regressions. The $F$-test of instrument exclusion is significant for all radii and is above the threshold-level of 10 (Staiger & Stock, 1997) for radii between 3 and 10 miles. Due to the highest explanatory power in the first-stage regression, we use the number of sports facilities within 6 miles as instrument in the main specification. The use of 6 miles as distance boundary is also consistent with an empirical finding by Pawlowski et al. (2009) that people are on average willing to spend a maximum of 28 minutes to travel to sport facilities. However, our results are widely robust to the use of alternative distance boundaries to construct the instrument (see Table A.2 in the Appendix).[5]

[4] The dichotomization avoids any functional form assumptions for different subgroup effects (Lechner, 2009). Of course, one could easily argue for counting occasional sports activity as being active. For example, Lechner (2009) has chosen a definition that separates less then monthly sports participation (inactive) and monthly sports participation (active). To test for the impact of our particular definition, we additionally estimated all our models with cut-off points that treat occasional sports participation as "active". The results are virtually unaffected by the alternative cut-off points (see Appendix, Table A.1).

[5] The only findings that do not hold for all boundaries between 4 and 8 miles are related to the effect of sports activity on sleeping problems and overweight. While the coefficients remain negative throughout all specifications, the effects become marginally insignificant for the 4-mile boundary for overweight and the for 7- and 8-mile boundaries for sleeping problems. This indicates that reduced power in the first stage significantly affects the precision in the second stage.

**Table 1**
Comparison of different measures of *proximity* to sports facilities.

| Distance boundaries | Mean | Std.dev. | First-stage $F$-test of excluded instrument |
|---|---|---|---|
| Sports facilities within 1 mile | 1.68 | 2.30 | $F$=2.71 |
| Sports facilities within 2 miles | 5.01 | 6.41 | $F$=8.19 |
| Sports facilities within 3 miles | 9.13 | 10.88 | $F$=19.15 |
| Sports facilities within 4 miles | 13.70 | 15.43 | $F$=24.48 |
| Sports facilities within 5 miles | 18.45 | 19.56 | $F$=30.68 |
| Sports facilities within 6 miles | 23.41 | 23.44 | $F$=33.71 |
| Sports facilities within 7 miles | 28.67 | 27.66 | $F$=27.51 |
| Sports facilities within 8 miles | 34.37 | 31.97 | $F$=30.26 |
| Sports facilities within 9 miles | 40.52 | 36.39 | $F$=29.02 |
| Sports facilities within 10 miles | 47.14 | 40.78 | $F$=21.66 |
| Number of households | 4,016 | | |
| Number of individuals | 6,558 | | |

**Notes:** Data on sports facilities is drawn from the 2008 Swiss *Business Census* and is linked to the home addresses of SHP individuals. The $F$-test of excluded instrument reflects the power of our measure of proximity to sports facilities in Eq. (1) presented in Section 3. All models control for age, gender, marital status, education, household income, household with children, community typology, and population density.

An illustrative example of our approach is shown in Figure 1 for an individual living in central Switzerland.

Of course, valid instruments not only have to be powerful. The exogeneity condition of IV regressions requires that instruments are not correlated with the error term in the second stage (see e.g., Stock & Watson, 2003; Murray, 2006). In our analysis, this means that proximity to sports facilities must be uncorrelated with health-related outcomes, except through variables that are included in the equation. Hence, we have to diligently check the control variables. More specifically, we have to control for factors that correlate with both proximity to sports facilities and health/health care utilization. Brunekreef and Holgate (2002), Passchier-Vermeer and Passchier (2000) and Boes, Nüesch, and Stillman (2013) show that living in urban areas is likely to affect individual health status through noise pollution, air pollution and other factors. At the same time, urban areas with a high population density naturally provide a higher number of sports facilities. Therefore, we include two measures of residential area characteristics for each individual home address in our analysis. These are

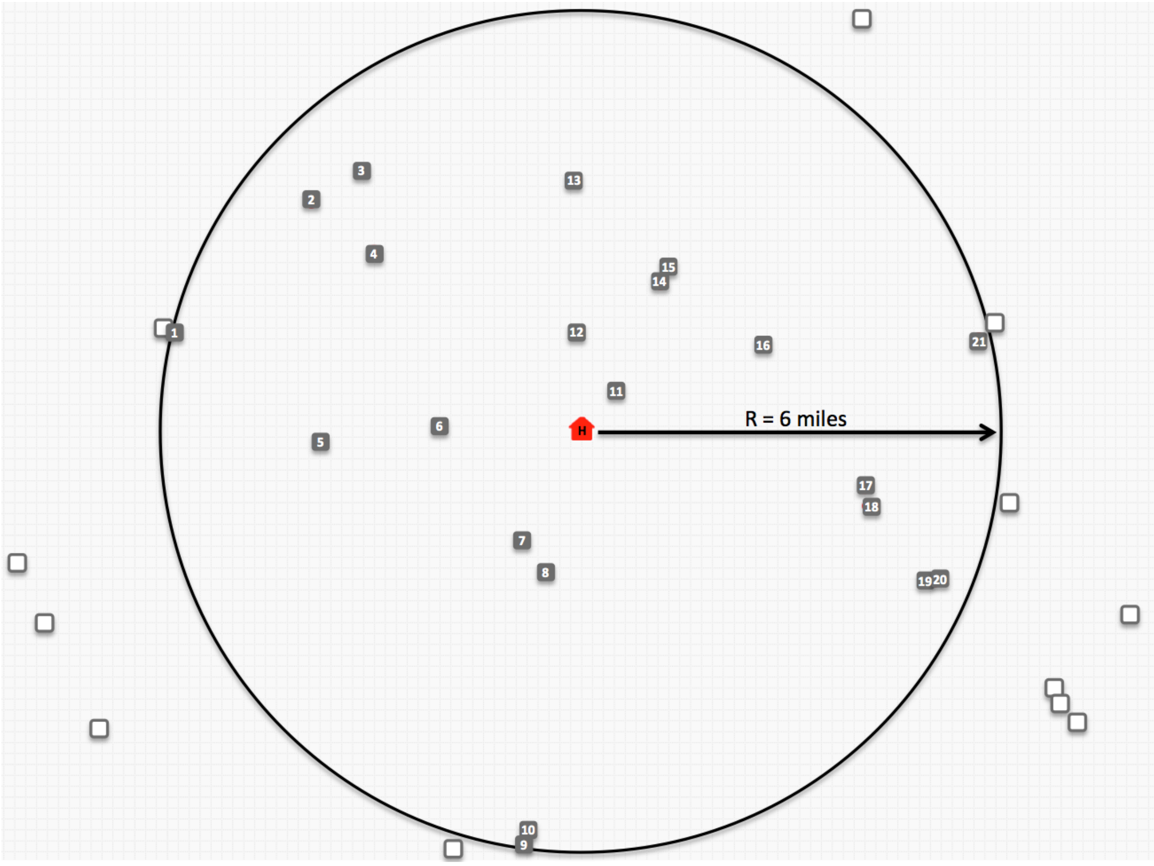


**Figure 1**
Measure of *proximity* to sport facilities.
**Notes:** The figure depicts a distance boundary of 6 miles for counting the number of sports facilities surrounding an individual's home address. Filled squares represent units that are included in the count measure (i.e., 21) and empty squares represent units that are treated as "out of reach".

the community typology (following the official SHP categorization) and population density[6] per square mile.

In addition, we include a large set of demographic and socio-economic control variables that may reflect individual differences in the availability of sports facilities and that have been widely used in studies on residential choice and in studies on health-related outcomes (see e.g., Winkelmann, 2004; Sari, 2009; Lee & Waddell, 2010;

[6]The data on home-address specific population density is obtained from the 1990, 2000, and 2010 Swiss *Population Census* through extrapolation for the year 2008. The *Population Census* counts all individuals in each and every hectare in Switzerland. We linked this hectare-based population data to the SHP households via Swiss grid coordinates. Population size is converted into population density per square mile based on the same distance boundary that is used for our proximity to sports facility measure (i.e., 6 miles in the main specification).

Kim, Pagliara, & Preston, 2005). These include age, sex, marital status, education, household earnings[7], and household with children.

Nevertheless, one must always be cautious with regard to the exogeneity condition of IV models because it is impossible to prove the null hypothesis of no correlation between instruments and the (unobserved) error term in the second stage. If more sporty people intentionally choose to live close to sports facilities, our IV estimates could still be biased (irrespective of our included controls). However, research on residential choice suggests that non-work travel preferences do not play an important role in neighbourhood selection and that considerations of accessibility are mainly driven by commuting to work (see the discussion in Chatman (2009)). For example, Lee, Waddell, Wang, and Pendyala (2010) estimated that commuting to work is more than ten times as important as the shopping opportunities in the neighbourhood for residential choice. Following this view, we believe that unobserved residential sorting based on proximity to sports facilities is not a major concern for our estimation strategy.

# 3 Estimation method

Because previous research has shown that non-linear IV models are potentially biased when estimated with standard two-stage least squares methods (see the discussion in Terza, Bradford, & Dismuke, 2008), we estimate a two-stage residual inclusion (2SRI) model. 2SRI is basically a version of the control function approach developed by Wooldridge (2002, 2014). Rather than replacing the endogenous explanatory variable

[7] Unfortunately, 7.9 percent of the individuals did not provide valid data for household earnings. In order to keep these observations in the sample, we classify respondents into five different income groups, one of which is "unknown".

with the first-stage predictors, the equation in the second stage includes the first-stage residuals as an additional regressor.

In the first stage of our 2SRI procedure, we identify the probability of an individual to participate in sports activities by the following model framework:

$$\textit{sports-activity}_i = f(\textit{proximity}_i, X_i, \omega_i), \tag{1}$$

where $i$ indexes the individual and $\omega$ denotes the random regression error term. The dependent variable *sports-activity*$_i$ is a dummy variable that is 1 for individuals that participate in sports activities at least once a week and 0 for individuals that do not. The variable *proximity*$_i$ captures the number of sports facilities within 6 miles of the home address of individual $i$. The vector of covariates $X_i$ captures a set of home address-specific residential area controls and observed individual background variables. The function $f(\cdot)$ will be a linear function in our main specification. Estimating the first stage by a linear probability model is the safest way when the underlying error distribution is unknown (Angrist, 2001) and allows us to compute the $F$-statistic of the excluded instrument.[8]

In the second stage, we model our outcome variables as a function of the endogenous dummy for weekly sports activity, the set of covariates, and the saved residuals of the regression in the first stage. The model framework is:

$$H_i = f(\textit{sports-activity}_i, X_i, \hat{\omega}_i, \nu_i), \tag{2}$$

[8] As a sensitive check, our 2SRI estimation was repeated using a Probit model in the first stage. The results, presented in Table A.3 of the Appendix, are widely unaffected by the use of a Probit specification. The only finding that does not hold for a Probit first stage is related to the effect of sports activity on overweight. While the coefficient remains strongly negative, the effect becomes marginally insignificant.

where $i$ again indexes the individual and $\nu_i$ denotes the random regression error term. $X_i$ denotes the vector of covariates from the first stage. The dependent variable $H_i$ represents our set of health-related outcomes variables. The main explanatory variable of interest is *sports-activity*$_i$. $\hat{\omega}_i$ denotes the residuals from the first-stage estimation and substitutes for any unobserved confounders that might be correlated with both *sports-activity*$_i$ and $H_i$.

For all binary outcome variables, we estimate the second stage using a a linear probability model.[9] For count outcome variables, we use negative binomial MLE.[10] To account for the fact that the second stage of our 2SRI model includes a regressor imputed from first-stage estimates, the coefficients' standard errors in the second stage are bootstrapped (Carpio, Wohlgenant, & Boonsaeng, 2008; Huang & Humphreys, 2012). A total of B = 999 replications were used to generate the standard errors, confidence intervals and hypothesis tests.[11]

# 4 Results

## 4.1 Summary statistics

Descriptive statistics for all our variables are shown in Table 2. Individuals reported on average 3.5 doctor visits and 0.9 hospital days within the 12-month period. There is a very long right tail of the response distribution for both measures but the proportion of reported zeros (indicating no use at all) is significantly higher for hospital use

[9] Additionally, we report Probit estimations for our binary outcome variables in the Appendix (see column (3) in Table A.3). The results are widely unaffected by the alternative specification of the second stage.

[10] We choose the negative binomial MLE over Poisson estimation because the number of doctor visits and the number of days in hospital are both overdispersed (Wooldridge, 2002).

[11] Since results using bootstrapped standard errors are not fully replicable by other researchers, we also estimated all our models with conventional (Huber-White) "robust" standard errors. We obtained very similar standard errors in both approaches, which resulted in identical inference for all outcome measures. Full results for estimations with conventional standard errors are available from the authors upon request.

than doctor use (85.3% compared to 24.9%).[12] The high share of hospital non-users also explains the low number of average hospital treatment days. 35.7 percent of the individuals are overweight while about 8 to 10 percent of the individuals report suffering from one of the other three health issues (sleeping problems, headaches, and back problems).

65.3 percent of the individuals in our sample do sports at least once a week. Compared to existing studies using samples from other countries, the proportion of active people in our sample is in the middle of the range of the observed numbers. While Huang and Humphreys (2012) found 76.5 percent of the individuals in a US sample to be physically active, other studies from Canada and Germany found only about 50 percent (Sari, 2009; Humphreys, McLeod, & Ruseski, 2014) or 40 percent (Lechner, 2009; Sari, 2014) to be active individuals. Our instrument (Number of sports facilities within 6 miles) has a mean value of 23.42. This indicates that individuals in our sample have on average 23.4 sports facilities within 6 miles of their place of residence. The measure has substantial variation as the number of sports facilities ranges from zero to 106 with a standard deviation of 23.40.

The average age in the sample is 46.2 years. A little under half the sample is male (44.4%) and a little over half the sample is married (53.8%). Individual education splits into five categories with shares between 10 percent and 36 percent. The high share of apprenticeships (36.4%) reflects the importance of occupational training in the Swiss education system. In terms of household income, individuals are divided into four income levels and a non-response group, with most individuals (32.6%) living in households with an income between 50,000 and 100,000 Swiss Francs (reflecting

[12]To avoid problems with outliers (some individuals reported up to 200 doctor visits or 327 hospital days), we "winsorized" the responses by setting outlying values to the 99th percentile. However, all our results are very robust to use of the original (non-winsorized) reported values. We only observe a slight increase in the size of the marginal effects using the original counts.

roughly \$48,000 - \$96,000 based on the currency rate of 2008). Most of the individuals live in urban centres (26.8%) or in a suburban type of community (30.5%). The average population density per square mile is 1,575.4.

**Table 2**
Summary statistics.

| Variables | Mean | Std.dev. | Min. | Max. |
|---|---|---|---|---|
| *Health care utilization:* | | | | |
| Number of doctor visits | 3.485 | 5.121 | 0 | 30 |
| Number of hospital days | 0.876 | 3.347 | 0 | 25 |
| *Health:* | | | | |
| Overweight | 0.357 | 0.479 | 0 | 1 |
| Sleeping problems | 0.085 | 0.279 | 0 | 1 |
| Headaches | 0.077 | 0.266 | 0 | 1 |
| Back problems | 0.100 | 0.300 | 0 | 1 |
| *Individual sports activity:* | | | | |
| Doing sports at least once a week | 0.653 | 0.476 | 0 | 1 |
| *Instrumental variable:* | | | | |
| Number of sports facilities within 6 miles | 23.415 | 23.440 | 0 | 106 |
| *Demographics and socio-economic controls:* | | | | |
| Age | 46.12 | 18.36 | 14 | 96 |
| Male | 0.445 | 0.497 | 0 | 1 |
| Married | 0.537 | 0.499 | 0 | 1 |
| Education: Compulsory | 0.228 | 0.420 | 0 | 1 |
| Education: Apprenticeship | 0.363 | 0.481 | 0 | 1 |
| Education: University-entrance diploma | 0.100 | 0.299 | 0 | 1 |
| Education: Post-apprenticeship diploma | 0.161 | 0.368 | 0 | 1 |
| Education: University degree | 0.148 | 0.355 | 0 | 1 |
| Household income: <50,001 Swiss Francs | 0.111 | 0.314 | 0 | 1 |
| Household income: 50,001–100,000 Swiss Francs | 0.325 | 0.469 | 0 | 1 |
| Household income: 100,001–150,000 Swiss Francs | 0.280 | 0.449 | 0 | 1 |
| Household income: >150,000 Swiss Francs | 0.206 | 0.405 | 0 | 1 |
| Household income: unknown | 0.078 | 0.268 | 0 | 1 |
| Household with children | 0.372 | 0.483 | 0 | 1 |
| *Residential area:* | | | | |
| Community typology: Centres | 0.269 | 0.444 | 0 | 1 |
| Community typology: Suburban | 0.306 | 0.461 | 0 | 1 |
| Community typology: Wealthy | 0.038 | 0.191 | 0 | 1 |
| Community typology: Periurban | 0.114 | 0.318 | 0 | 1 |
| Community typology: Touristic | 0.023 | 0.149 | 0 | 1 |
| Community typology: Industrial | 0.088 | 0.284 | 0 | 1 |
| Community typology: Rural | 0.079 | 0.270 | 0 | 1 |
| Community typology: Agricultural | 0.084 | 0.277 | 0 | 1 |
| Population density per square mile | 1,575.0 | 1,370.2 | 9 | 5,737 |
| Number of households | 4,016 | | | |
| Number of individuals | 6,558 | | | |

**Notes:** Data on sports facilities is drawn from the 2008 Swiss *Business Census*. Data on population density is interpolated from the 1990, 2000, and 2010 Swiss *Population Census*. All other variables are directly drawn from the 2008 SHP survey. Number of doctor visits and Number of hospital days are “winsorized” to the 99th percentile.

## 4.2 First-stage results

The first-stage results show that the number of sports facilities within 6 miles significantly increases weekly sports activity (see Table 3). The *F*-statistic for excluding the number of sports facilities in the regression is 33.71, indicating that our instrument easily passes the conventional test for power in the first stage (Staiger & Stock, 1997). This implies that proximity to sports facilities strongly predicts individual sports activity.

The estimates for the additional demographic and socio-economic controls are largely consistent with previous research on the determinants of individual sports activity (see e.g., Huang & Humphreys, 2012; Farrell & Shields, 2002). The likelihood of sports activity strongly increases with education and household income and decreases with age. Interestingly, we observe males to be less active then women in the Swiss context, while an earlier study from England showed the opposite relationship between gender and sports activity (Farrell & Shields, 2002). With regard to the residential area, we find that sports activity is higher in suburban areas than in centres and that sports activity decreases with a higher population density.

## 4.3 Regression results

Table 4 presents the estimates of the effect of weekly sports activity on health care utilization (Panel A) and health (Panel B). To consider the endogeneity of sports activity, we estimate IV models using 2SRI. These models include the control variables from the first-stage regression and the first-stage residuals as an additional regressor. For the purpose of comparison, Table 4 also presents non-IV models that exclude the first-stage residuals. Column (1) reports the estimated coefficients of weekly sports

**Table 3**
First-stage results: probability of an individual to do sports at least once a week.

| Variables | LPM M1 |
|---|---|
| Number of sports facilities within 6 miles | 0.006*** |
| | (0.001) |
| Age | –0.004*** |
| | (0.0004) |
| Male | –0.025** |
| | (0.012) |
| Married | –0.009 |
| | (0.015) |
| Education: Compulsory | Ref. group |
| Education: Apprenticeship | 0.030* |
| | (0.016) |
| Education: University-entrance diploma | 0.036* |
| | (0.022) |
| Education: Post-apprenticeship diploma | 0.080*** |
| | (0.020) |
| Education: University degree | 0.108*** |
| | (0.020) |
| Household income: <50,001 Swiss Francs | Ref. group |
| Household income: 50,001–100,000 Swiss Francs | 0.084*** |
| | (0.022) |
| Household income: 100,001–150,000 Swiss Francs | 0.153*** |
| | (0.024) |
| Household income: >150,000 Swiss Francs | 0.197*** |
| | (0.025) |
| Household income: unknown | 0.135*** |
| | (0.029) |
| Household with children | –0.009 |
| | (0.014) |
| Community typology: Centres | Ref. group |
| Community typology: Suburban | 0.043*** |
| | (0.015) |
| Community typology: Wealthy | 0.080*** |
| | (0.030) |
| Community typology: Periurban | 0.057*** |
| | (0.020) |
| Community typology: Touristic | –0.020 |
| | (0.042) |
| Community typology: Industrial | –0.022 |
| | (0.024) |
| Community typology: Rural | 0.017 |
| | (0.025) |
| Community typology: Agricultural | 0.017 |
| | (0.024) |
| Population density per square mile/100 | –0.010*** |
| | (0.002) |
| Observations | 6,558 |
| *F*-test of excluded instrument | 33.71 |

**Notes:** In Model M1, OLS estimates for Eq. (1) are displayed. The dependent variable is a dummy variable that takes a value of 1 for individuals that do sports at least once a week and a value of 0 otherwise. All estimations also included a constant (not reported). Robust standard errors are given in parentheses. *, **, and *** denote statistical significance at the 10%, 5%, and 1% levels, respectively.

activity from the non-IV models and column (2) reports the estimated coefficients of weekly sports activity using the 2SRI approach.

Because raw coefficient estimates from negative binomials are difficult to interpret (Dávalos, Fang, & French, 2012), we also report the incidence rate ratio (IRR) for models with count outcomes (i.e., the health care utilization models in Panel A). An IRR represents the difference in the rate of the count outcome predicted by the model when switching sports activity from zero to one while all other variables are kept constant at their means. A value greater than one indicates that sports activity increases the outcome, and a value between zero and one indicates that sports activity decreases the outcome. The further away from one a value is, the stronger the effect becomes.

The results from the non-IV models show that sports activity significantly reduces the number of doctor visits, the number of hospital days, overweight, sleeping problems, headaches, and back problems (see column (1)). Controlling for the endogeneity of sports activity with the 2SRI approach results in different findings. Column (2), Panel A also shows that sports activity significantly reduces the number of doctor visits but the magnitude of the effect is much larger when controlling for the endogeneity of sports activity. We observe an IRR of 23 percent in the IV model compared to an IRR of 88 percent in the non-IV model. The greater distance of the IRR from one in the IV model indicates a bias toward zero in the non-IV results. Similarly, we find that sports activity reduces the number of hospital days at a higher rate when using an IV model than when using a non-IV model (IRR of 43% compared to an IRR of 73%). However, the effect of sports activity on hospital days is not statistically significant in the IV model due to the high standard errors.

**Table 4**
Regression results: effects of weekly sports activity on health care utilization and health.

| | Non-IV (1) | IV (2SRI) (2) |
|---|---|---|
| Panel A: health care utilization outcomes (Negbin) | | |
| Number of doctor visits | –0.132*** [0.876] | –1.475*** [0.229] |
| | (0.039) | (0.531) |
| Number of hospital days | –0.317*** [0.728] | –0.840 [0.432] |
| | (0.098) | (1.438) |
| Panel B: health outcomes (LPM) | | |
| Overweight | –0.102*** | –0.280* |
| | (0.012) | (0.165) |
| Sleeping problems | –0.031*** | –0.230** |
| | (0.008) | (0.104) |
| Headaches | –0.023*** | 0.045 |
| | (0.007) | (0.100) |
| Back problems | –0.045*** | 0.097 |
| | (0.008) | (0.117) |
| Observations | 6,558 | 6,558 |
| *F*-test of excluded instrument in the first stage | – | 33.71 |

**Notes:** Non-IV estimates for weekly sports activity are displayed in column (1) with white robust standard errors in parentheses. IV estimates for weekly sports activity are displayed in column (2) with bootstrapped standard errors (999 reps) in parentheses. In Panel A, negative binomial MLE is used and incidence rate ratios are displayed in brackets. In Panel B, OLS estimates are displayed. All models control for age, gender, marital status, education, household income, household with children, community typology, and population density. All estimations also included a constant (not reported). *, **, and *** denote statistical significance at the 10%, 5%, and 1% levels, respectively.

Column (2), Panel B shows that sports activity significantly reduces the probability of suffering from overweight and sleeping problems, by 28 percent and 23 percent, respectively. Again, the magnitudes of the effects are larger when controlling for the endogeneity of sports activity, indicating a bias toward zero in the non-IV results. In contrast, the effects of sports activity on headaches and back problems become statistically insignificant (and positive) in the IV models. Thus the findings from the non-IV models seem to be spurious, because the effects from sports activity on headaches and back problems disappear once the endogeneity of sports activity is controlled for.

# 5 Discussion and conclusion

This paper uses a representative sample of the Swiss population and geocoded data on sports facilities, sports activity, health and health care utilization to estimate the causal effect of sports activity on health and health care utilization. Unlike previous correlational studies, we use an instrument for sports activity, namely the number of sports facilities within 6 miles of the individual's place of residence. We find that sports activity significantly reduces doctor visits, overweight and sleeping problems. Though the same trends are seen when we use an estimation without the instrumental variable and have been seen in previous correlational studies, the magnitudes of these effects are considerably larger in our IV estimations. Our results may be useful for estimating the cost-effectiveness of sports facilities to encourage sports activity as a way of reducing health problems and health care utilization.

Two reasons may explain why our IV estimates on health care utilization, overweight and sleeping problems are higher than the effects reported in the previous literature: first, our IV method addresses measurement error in the self-reported sports activity variable (see Ferrari et al. (2007) for reporting errors of physical activity). Reporting errors in sports activity lead to an underestimation of the effect. Second, previous estimates of sports activity on health care utilization may have suffered from an omitted variable bias. For example, sporty people may be more health-conscious, which increases health care utilization and perceived health problems even in the absence of obvious health issues. For example, previous evidence has shown that higher levels of body awareness are associated with more patient-initiated visits to HMO and patient-initiated contacts with hospital emergency rooms among older adults (Hansell et al., 1991). Also, health-conscious subgroups of the population are more likely to participate in screening-related health care utilization (Ioannou et al., 2003).

The endogeneity of sports activity is also important when estimating its effects on back problems and headaches. The correlational estimation that only considers a set of control variables associates sports activity with a small but significant reduction in the frequency of back problems and headaches, whereas the IV estimates associate sports activity with a small and statistically insignificant increase in the frequency of these health issues. The non-IV results seem to be negatively biased and are likely to suffer from a reverse causation issue. Individuals with back problems and headaches (probably more than individuals with sleeping problems and overweight) tend to reduce sports activity.

This paper has some limitations. The first limitation concerns the validity of the instrument *proximity to sports facilities*. Our identifying assumption is that the proximity to sports facilities is uncorrelated with unobserved determinants of health and health care utilization. We argue that this assumption is plausible for three reasons: first, because sports facilities are provided by communities and not by individuals, reverse causality can be excluded. Second, community-level variables help to control for potential confounders that are likely to be correlated with both the number of sports facilities and health and health care utilization. Third, it is well-known that individuals self-select into neighborhoods based on housing prices, housing quality, commuter distance, school quality and/or environmental factors such as noise. However, non-work related travel distances (such as proximity to sports facilities) are found to play only a negligible role in selecting a neighbourhood to live in (Lee et al., 2010; Chatman, 2009). Nevertheless, as residential neighborhoods are not randomly assigned, we cannot completely rule out that unobserved health determinants could influence residential sorting into neighborhoods with few or many sports facilities. Future studies should conduct field experiments with representative samples out of which a random subgroup is incentivized to participate in sports.

A second limitation is that we use a cross-sectional data set. While panel data on sports facilities, individual sports activity and health-related outcomes are available, the variation of the number of sports facilities over time is too low to have any statistical power in first-stage regressions. Therefore, the application of an instrumental variables strategy in fixed-effects models is not feasible (see Table A.4 in the Appendix).

A third limitation is that the data on sports activity and health-related outcomes is self-reported. Although the IV method helps to correct for reporting errors in the sports activity measure, it does not eliminate reporting errors in the outcome variables. A fourth limitation of our study is that we have data on sports activity only at the consolidated level for all different types of sports and at the ordinal level for the frequency of sports. Ideally, we would have data on subgroups of sports (e.g., football, tennis, jogging) and hours of weekly participation that would allow us to estimate marginal effects of additional hours in different types of sports.

Despite these limitations, this paper makes an important contribution by providing first IV estimates on the effects of sports activity on health and health care utilization based on a representative sample. For doctor visits, overweight, and sleeping problems, the magnitudes of the causal effects are higher than indicated by correlations between sports activity and these outcomes, indicating that measurement error and omitted variables bias can lead to an underestimation of the associations in correlational studies.

# Appendix

**Table A.1**
Effects of sports activity on health care utilization and health: alternative cut-offs for being "active".

| | IV<br>(1)<br>(>Weekly (original)) | IV<br>(2)<br>(>Monthly) | IV<br>(3)<br>(>Never) |
|---|---|---|---|
| Panel A: health care utilization outcomes (Negbin) | | | |
| Number of doctor visits | –1.475*** [0.229] | –1.537*** [0.215] | –1.591*** [0.204] |
| | (0.531) | (0.550) | (0.554) |
| Number of hospital days | –0.840 [0.432] | –0.938 [0.391] | –0.909 [0.403] |
| | (1.438) | (1.535) | (1.515) |
| Panel B: health outcomes (LPM) | | | |
| Overweight | –0.280* | –0.292* | –0.300* |
| | (0.165) | (0.168) | (0.175) |
| Sleeping problems | –0.230** | –0.240** | –0.247** |
| | (0.104) | (0.112) | (0.115) |
| Headaches | 0.045 | 0.047 | 0.048 |
| | (0.100) | (0.100) | (0.105) |
| Back problems | 0.097 | 0.102 | 0.105 |
| | (0.117) | (0.124) | (0.128) |
| Observations | 6,558 | 6,558 | 6,558 |
| $F$-test of excluded instrument in the first stage | 33.71 | 35.62 | 36.32 |

**Notes:** IV (2SRI) estimates for sports activity are displayed with bootstrapped standard errors (999 reps) in parentheses. In Panel A, negative binomial MLE is used and incidence rate ratios are displayed in brackets. In Panel B, OLS estimates are displayed. Column (1) to (3) refer to different sports activity thresholds for individuals to be categorized as "active". All models control for age, gender, marital status, education, household income, household with children, community typology, and population density. All estimations also included a constant (not reported). *, **, and *** denote statistical significance at the 10%, 5%, and 1% levels, respectively.

**Table A.2**
Effects of weekly sports activity on health care utilization and health: using a range of distance boundaries for proximity to sports facilities.

| | IV (1) (4 miles) | IV (2) (5 miles) | IV (3) (6 miles (original)) | IV (4) (7 miles) | IV (5) (8 miles) |
|---|---|---|---|---|---|
| Panel A: health care utilization outcomes (Negbin) | | | | | |
| Number of doctor visits | –1.487** [0.226] | –1.764*** [0.171] | –1.475*** [0.229] | –1.346** [0.260] | –1.469*** [0.230] |
| | (0.630) | (0.566) | (0.531) | (0.561) | (0.559) |
| Number of hospital days | –0.502 [0.605] | –0.691 [0.501] | –0.840 [0.432] | –1.510 [0.221] | –1.257 [0.284] |
| | (1.613) | (1.528) | (1.438) | (1.577) | (0.154) |
| Panel B: health outcomes (LPM) | | | | | |
| Overweight | –0.211 | –0.290* | –0.280* | –0.340** | –0.354* |
| | (0.193) | (0.174) | (0.165) | (0.180) | (0.183) |
| Sleeping problems | –0.334*** | –0.321*** | –0.230** | –0.178 | –0.180 |
| | (0.126) | (0.111) | (0.104) | (0.118) | (0.114) |
| Headaches | –0.007 | 0.059 | 0.045 | 0.056 | 0.067 |
| | (0.112) | (0.101) | (0.100) | (0.107) | (0.104) |
| Back problems | 0.029 | 0.026 | 0.097 | 0.148 | 0.129 |
| | (0.135) | (0.124) | (0.117) | (0.126) | (0.124) |
| Observations | 6,558 | 6,558 | 6,558 | 6,558 | 6,558 |
| Number of sports facilities within distance boundary | 13.70 | 18.45 | 23.41 | 28.67 | 34.37 |
| Instrument coefficient in the first stage | 0.006*** | 0.006*** | 0.006*** | 0.005*** | 0.005*** |
| *F*-test of excluded instrument in the first stage | 24.48 | 30.68 | 33.71 | 27.51 | 30.26 |

**Notes:** IV (2SRI) estimates for weekly sports activity are displayed and bootstrapped standard errors (999 reps) are given in parentheses. In Panel A, negative binomial MLE is used and incidence rate ratios are displayed in brackets. In Panel B, OLS estimates are displayed. Column (1) to (5) refer to different distance boundaries for proximity to sports facilities. All models control for age, gender, marital status, education, household income, household with children, community typology, and population density. All estimations also included a constant (not reported). *, **, and *** denote statistical significance at the 10%, 5%, and 1% levels, respectively.

**Table A.3**
Effects of weekly sports activity on health care utilization and health: alternative estimation approach.

| | IV (First stage: LPM) | IV (First stage: Probit) | |
|---|---|---|---|
| | (1) (Negbin/LPM (original)) | (2) (Negbin/LPM) | (3) (Negbin/Probit) |
| Panel A: health care utilization outcomes | | | |
| Number of doctor visits | –1.475*** [0.229] | –1.553*** [0.212] | –1.553*** [0.212] |
| | (0.531) | (0.512) | (0.512) |
| Number of hospital days | –0.840 [0.432] | –0.957 [0.384] | –0.957 [0.384] |
| | (1.438) | (1.444) | (1.444) |
| Panel B: health outcomes | | | |
| Overweight | –0.280* | –0.253 | –0.667 [–0.250] |
| | (0.165) | (0.160) | (0.483) |
| Sleeping problems | –0.230** | –0.213** | –1.284* [–0.252] |
| | (0.104) | (0.107) | (0.659) |
| Headaches | 0.045 | 0.090 | 0.726 [0.083] |
| | (0.100) | (0.097) | (0.710) |
| Back problems | 0.097 | 0.094 | 0.515 [0.076] |
| | (0.117) | (0.117) | (0.630) |
| Observations | 6,558 | 6,558 | 6,558 |

**Notes:** IV (2SRI) estimates for weekly sports activity are displayed and bootstrapped standard errors (999 reps) are given in parentheses. In column (1), included residuals are obtained from a linear probability first-stage regression. In column (2) and (3), included residuals are obtained from a Probit first-stage regression. In column (1) and (2), OLS estimates are displayed for the binary outcomes in Panel B. In column (3), Probit estimates are displayed for the binary outcomes in Panel B (with marginal effects at the mean in brackets). For all models in Panel A, negative binomial MLE is used and incidence rate ratios are displayed in brackets. All models control for age, gender, marital status, education, household income, household with children, community typology, and population density. *, **, and *** denote statistical significance at the 10%, 5%, and 1% levels, respectively.

**Table A.4**
First-stage results: individual fixed-effects model with panel data from 1999 until 2008.

| Variables | LPM FE1 |
|---|---|
| Number of sports facilities within 6 miles | –0.0001 |
| | (0.0006) |
| Demographic and socio-economic control variables | Yes |
| Residential area control variables | Yes |
| Individual fixed-effects | Yes |
| Observations | 65,909 |
| Number of individuals | 14,574 |
| *F*-test of excluded instrument in the first stage | 0.01 |

**Notes:** The estimation included 10 years of individual panel data from 1999 to 2008. The data on sports facilities is drawn and interpolated from the 1998, 2001, 2005, and 2008 Swiss *Business Census*. Data on population density is drawn and interpolated from the 1990, 2000, and 2010 Swiss *Population Census*. All other variables are directly drawn from the SHP surveys 1999–2008. In Model FE1, OLS estimates for Eq. (1) including individual fixed-effects are displayed. The dependent variable is a dummy variable that takes a value of 1 for individuals that do sports at least once a week and a value of 0 otherwise. The estimation also included a constant (not reported). *, **, and *** denote statistical significance at the 10%, 5%, and 1% levels, respectively.

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# Curriculum vitae

### Personal information

Name: Marc Brechot
Date of birth: February 12, 1983
Nationality: Swiss

### Education

| | |
|---|---|
| Ph.D. studies in Business Administration<br>University of Zurich | 2/2011 – 4/2015 |
| Licenciate in Business Administration<br>University of Zurich<br>Graduation: lic.oec.publ. | 9/2003 – 5/2009 |

### Professional experience

| | |
|---|---|
| Teaching and research associate<br>University of Zurich, Department of Business Administration | 1/2011 – dato |
| Financial analyst *(internship)*<br>Globalance Capital AG | 8/2009 – 8/2010 |
| Associate *(part time)*<br>Swiss Re, Actual Services: Product Development & Strategy | 8/2006 – 3/2007 |
| Associate *(part time)*<br>Credit Suisse, Asset Management: Institutional Sales | 2/2005 – 9/2005 |

### Teaching

| | |
|---|---|
| Mandated teaching staff - University of Zurich<br>Strategic Management *(Tutorial)*, Governance *(Seminar)* | 1/2011 – dato |